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THE AMERICAN JOURNAL OF PSYCHIATRY

(FORMERLY THE AMERICAN JOURNAL OF INSANITY)

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BROMIDE DELIRIUM AND OTHER BROMIDE PSYCHOSES.*

By MAX LEVIN, M. D.,
Clinical Director, Harrisburg State Hospital, Harrisburg, Pa.

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Summary.

Bromide delirium has been known for many years, but only since 1925 has it been susceptible of adequate study. In that year Walter and Hauptmann described a method for determining the concentration of bromides in the serum. Prior to the invention of this method the recognition of bromide delirium was as a general rule possible only when one had accurate knowledge of the bromide intake, of its dosage, and of its relation in point of time with the onset of the delirium. For obvious reasons, under the ordinary conditions of clinical practice, many cases were overlooked. Today, with a simple chemical test one may easily determine the existence of intoxication, and thus the recognition of bromide delirium has been vastly facilitated.

SCOPE OF PRESENT STUDY.

Merely to recognize that a patient is in a delirium requires but a few minutes of conversation with him, during which one finds that

*Read at the eighty-eighth annual meeting of The American Psychiatric Association, Philadelphia, Pa., May 30-June 3, 1932.

The accounts of the cases reported herein were brought as nearly up to date as possible in October, 1932.

the patient is (invariably) disoriented and (usually) restless and disturbed by hallucinations. The aim of the present study was to go beyond this point. Many hours were spent with the patients with the aim of discovering whether there were deviations in thinking other than those commonly known to occur in delirium. Moreover the patients had ample opportunity to express themselves in regard to subjectively recognized deviations from their normal feelings. In these ways we were provided with facts that are to some extent illuminating. We shall first present these facts and consider their significance.

This study deals with seven cases of delirium and one case of hallucinosis, admitted to the Harrisburg State Hospital during the first eleven months of the fiscal year beginning June 1, 1931. The total number of first admissions during this period was 223.

The technical procedure of the blood bromide test used by us is described in Diethelm's paper.

RELATION OF BROMIDE PSYCHOSES TO OTHER PSYCHOSES.

Bromide psychoses are almost always found to be superimposed on some pre-existing affection, since people rarely take bromides without cause. Of our eight patients, two had epilepsy beginning late in life; two were neurotic; one had hypertension and was worried about it; one had very mild senile symptoms; one, a drinker, had suffered a concussion before getting bromides; and one had a psychosis concerning which we unfortunately know very little.

Obviously not every psychotic patient with bromides in his blood has a bromide psychosis. In making routine blood examinations on newly admitted patients at the Harrisburg State Hospital, we have frequently found high bromide concentrations in patients with schizophrenia and other psychoses. The issue always is: Did the psychosis now present begin before or during the intoxication? If in an attempt to control the symptoms of a depression the patient is given bromides, and there is no subsequent change in the psychotic picture, the case cannot be regarded as one of a bromide psychosis. On the other hand, if following the administration of the drug the depressive picture gives way to a delirium, the possibility of a superimposed bromide delirium must be considered.

Bromide delirium, as is well known, clears up from two to six weeks after the discontinuance of the drug. Accordingly when a delirium, having supervened in a bromide-loaded individual, clears up several weeks after discontinuance, the diagnosis of bromide delirium may be considered established. Two considerations must be borne in mind. (1) An underlying major psychosis may persist after the termination of the delirium. In such a case the psychiatrist, upon superficial study, seeing the patient apparently unimproved, may mistakenly dismiss from his mind the possibility of a bromide delirium. Closer study of such a case will show that the *delirium* has cleared up, and that what remains is the psychosis which antedated the delirium. (2) It seems theoretically possible that a bromide intoxication may give rise to a *chronic* psychosis, so that a patient who was non-psychotic before the delirium, fails to return to his normal health even many months after the bromides have been discontinued. This possibility will be discussed below, in connection with Case 5.

SEVEN CASES OF BROMIDE DELIRIUM.

CASE 1.—*Summary. Bromide delirium superimposed on beginning senility.* Mrs. A., age 67, had for some years been taking large quantities of bromo-seltzer. She was admitted July 14, 1931, in a typical delirium. There was marked cyanosis (evidently due to methemoglobinemia, since there were signs of only slight myocardial failure). Only slight retinal arteriosclerosis was present. During her third week in the hospital the delirium cleared up completely. Subsequent examinations showed her to be an alert, jolly woman, whose only psychic abnormality was a moderate degree of forgetfulness. She was paroled September 17, 1931. When she was seen for follow-up purposes one year later, her general condition was excellent. She was no longer cyanotic, the cyanosis having disappeared about one month after her parole.

Comment.—Our laboratory did not begin to make blood bromide examinations until the end of October, 1931. Even without the blood examination, however, the facts in this case justify the diagnosis of bromide delirium: the delirium came on during bromide medication and cleared up during the third week after discontinuance. Moreover, the cyanosis is confirmatory evidence that the quantity of ingested bromo-seltzer had been considerable.

CASE 2.—*Summary. Bromide delirium superimposed on late epilepsy and followed by a temporary hallucinatory-paranoid aftermath.*

Mr. B., a well-adjusted pyknic man of steady habits, began at 48 to have epileptic seizures (July, 1928). At the same time his work ability declined, and in 1929 he was demoted to a less responsible position in the steel plant,

where until then he had been a chief clerk. In April, 1930, he was dismissed. In January, 1931, his wife noticed that his previously keen memory was failing. On May 26, 1931, he had severe *status epilepticus* and was unconscious until the next day. On June 4 he began to take a patented bromide remedy for epilepsy. He took this medicine every day until admission. In July the first imaginations occurred. In August he became more and more mixed up, and on September 13, 1931, he was admitted in a typical delirium. In the latter half of October there was noticeable improvement in his orientation and in his fears and hallucinations. He became permanently well oriented in the middle of November.

A remarkable feature of the case is that in the middle of October the delirium merged into a temporary hallucinatory-paranoid aftermath during which there was gross impairment of memory.

Hallucinatory-Paranoid Aftermath.—The delirium merged imperceptibly into the subsequent hallucinatory-paranoid state. During the delirium—*i. e.*, until the middle of October—the patient almost always misnamed the place ("Lewistown," etc.). After the middle of October, when he was no longer "irrational" in his general behavior and talk, he continued to be disoriented, but in a different way than formerly. Formerly he gave the name of the place incorrectly, *and did not realize that we considered his answers incorrect.* By contrast, he now realized that the people in his environment spoke of the place as the Harrisburg State Hospital, *but he suspiciously doubted the sincerity of their statement.* Thus, on numerous occasions during the second half of October and the first half of November, when I asked him the name of the place, he replied, "*You* say it's the Harrisburg State Hospital," clearly implying that he did not believe me. At the same time, he himself was not sure what city it was. On November 3 he remarked to me, "They tell me today is election day in York—*this* is York, isn't it?—not that I question your word, but I really honestly don't believe I'm in Harrisburg." (Why is it easier to believe that this is York and not Harrisburg?) "I've been in Harrisburg several times but I never saw any part of the town resembling this. Frankly, I don't *know* where I am." On November 18, when he already believed once and for all that he *was* in the Harrisburg State Hospital, he told me that he had previously often wondered why I was telling him it was Harrisburg when really (as he had then thought) it was some other town.

Such disorientation (I shall call it "paranoid disorientation," in contrast with delirious disorientation) will be discussed further in connection with Case 4.

Much more striking paranoid symptoms were observed by the relatives, to whom the patient at this stage spoke more freely than to the physicians. On November 11, when they sat with him in the corridor of the ward, he looked around suspiciously, as though he was afraid he would be overheard. Several times he signalled to them, to caution them not to speak too loud. On December 16 (when he had already been perfectly oriented for a month), he again told them that he and they were being overheard. He said, "We had better not talk so loud. The walls have ears. There are

dictaphones here and they take down what you say and place it on a record later on. We must be careful what we say."

The hallucinations of this period were invariably auditory. With one exception they consisted of hearing his name called. The patient's reaction was to go to the ward office and ask "Does anybody want me?" or "Didn't you call me?" When the attendants informed him that they had not called him, he usually seemed puzzled. The one exception referred to occurred on December 11, at a moving picture performance for the patients. After the audience were in their seats, the patient "heard" an announcement, "We'll not start this picture until John B——— gets out of this building."

The hallucinatory-paranoid aftermath cleared up at the end of December.

Memory.—Following the delirium gross impairment of memory was in evidence. Thus, although a devoted family man, he was unable to give the correct order of his seven children and the name of his only grandchild. On November 11 he retained only five digits. In December and thereafter the gross memory impairment was no longer demonstrable, and he now retained six digits. (His memory, to be sure, was not quite as strong as it had been prior to the onset of the epilepsy.)

The first blood bromide examination was on October 29, 1931. At that time, *over six weeks after discontinuance,* the serum content was 100 milligrams per 100 cubic centimeters.* On November 5 the serum contained 75 mgm.

It is noteworthy that from May 26, 1931, until October 15, 1931, no epileptic attacks occurred. Probably this is attributable to the then high concentration of bromides. On October 15 he had an epileptic attack, and began then to receive one and a half grains of luminal daily. On October 26 he had another attack, immediately after which a bilateral Babinski was noted. The luminal was increased to three grains daily and he had no further attacks. He was paroled on April 2, 1932.

Letters from the patient and his wife indicated that he felt "fine." I saw him on June 10, 1932; he looked hale and hearty, his only complaint being an occasional headache. There was no vomiting and the optic discs appeared normal. He was taking three grains of luminal daily and had had no attacks. Of course he did not have the keen mentality that he had had prior to the onset of the epilepsy. When the hospital social worker visited him, on September 9, 1932, she found him continuing to look and feel well.

*It is perhaps unnecessary to point out that a time factor enters into the interpretation of the blood bromide test. A concentration of 100 mgm. per 100 c. c. six weeks after discontinuance means a great deal; the same concentration a few *days* after discontinuance would mean comparatively little. Typical curves of elimination may be found in the papers of Wuth and Diethelm.

CASE 3.—*Summary. Bromide delirium superimposed on a state of chronic ill health (partly of somatic and partly of psychoneurotic origin).*

Mrs. C. began at 28 to show hypochondriacal preoccupations. She went from one doctor to another and had numerous operations. At 39 hysterectomy was performed. At 47 her worries about her health increased. She took every conceivable medicine, and practically every one of her numerous physicians gave her bromides. During the summer of 1931 (she was now 53 years old) she began to imagine things. On October 1, 1931, she was admitted in a typical delirium. On November 5 she showed the first signs of clearing, and on November 10 she became permanently clear. We were then able to see her as an intelligent but very neurotic woman.

The first blood bromide examination was on October 22—at least 22 days after the discontinuance of the drug. This examination showed 125 mgm. per 100 c. c.

The patient was paroled on December 12, 1931. When visited by the hospital social worker, on May 13, 1932, she looked well and had gained weight. She was doing her own housework.

CASE 4.—*Summary. Bromide delirium superimposed on a pre-existing major psychosis. The delirium was followed by a chronic hallucinatory-paranoid state.*

Mrs. D. was unhappily married at the age of 35 to a philanderer. She bore her troubles bravely until approximately July 1, 1931, when (age 48) she began to be depressed and was no longer able to conceal her unhappiness. On approximately July 25, 1931, she began to have delusions, thinking "they" were going to kill her. On August 1, 1931, she was admitted to a private sanitarium, where she remained two months. A report from this sanitarium states that she was excited and had delusions and hallucinations. No mention is made of orientation. On October 3, 1931, she was transferred to the Harrisburg State Hospital. Examination at this time showed a typical delirium. She rambled in her talk; the *place* was the court-house, the hotel in York (York being the town near which she resides), a building in Gettysburg, etc.; the various *physicians* she identified as business men of her acquaintance; the *month* she gave correctly, the *year* always as 1933. She wept, saying that today she was to get married, and we were detaining her. She heard much shooting going on outside and had other auditory illusions and hallucinations. She spoke of "talking through the air," by which she meant that with her mouth shut and without phonating she was able to communicate audible messages to persons far away, and was in turn able to hear similar messages coming from them.

The first blood examination, on October 29 (at least 26 days after the discontinuance of bromides), showed 125 mgm. per 100 c. c.

On November 8, 1931, the patient gave the first clear indication of emerging from the delirium. She smiled brightly and recognized that a change for the better had occurred "last week." Her previous condition she described as "mind trouble." (How did it show?) "Forgetfulness and wandering of the mind and *not knowing where I am.*"

The patient's condition subsequently showed radical differences from that prior to November 8. In contrasting these two conditions I shall conveniently speak of the period prior to November 8 as "the delirium" and of that after that date as "the subsequent paranoid state."

Knowledge of Place.—During the delirium, when asked to name the place, she *unhesitatingly* replied "York," "Gettysburg," "the hotel," etc. During the subsequent paranoid state *she gave none of these wrong answers, even though she doubted that this is Harrisburg.* Thus, on January 12, 1932, when asked to name the place, she replied, "They say this is Harrisburg, but I'm not sure." (Why do you doubt it?) "Because I haven't seen much of the city. It *could* be Harrisburg, but I can't say. *If I could get out among people, maybe I could find out.*" She felt that information obtained from people on the outside was more reliable than that obtained from insiders.

Knowledge of Persons.—During the delirium the patient *unhesitatingly* named incorrectly the persons about her. During the subsequent paranoid state she answered questions more warily. Thus, on November 25, 1931, I asked her who I am. She replied, "Some say you are a doctor," indicating by her tone that she doubted it. When I produced medical instruments, she said, "You must be a doctor then," but still in a tone of doubt.

Knowledge of the Year.—The most striking paranoid thinking was shown in reference to the year. The patient stated she had left home in August (which was correct), but she invariably insisted it had been August, 1933, instead of 1931. Accordingly throughout the remainder of 1931 she believed it was 1933, and at the turn of the year she kept insisting it was now 1934. She was aware of the discrepancy and had a characteristically paranoid explanation for it: we told her it was 1931 merely to see whether we could deceive her, our motive being to test her mind. Letters post-marked 1931 proved nothing; they might be several years old, even though they looked fresh and unrumpled. The date-line on the newspapers proved nothing; it would be comparatively simple to date incorrectly the papers destined for the hospital. I asked her what she would think if she went into town and read the date of a newspaper bought at a news-stand of her own choice; again she thought it would not be impossible to have the matter prearranged. She saw automobiles bearing 1932 tags, but thought it quite possible that false tags had been put on each car entering the hospital grounds. She knew she was not an important person and she admitted there was no reason why state officials and automobilists should conspire in the use of false tags, yet her belief in this possibility remained unshaken.

Hallucinations.—Throughout the subsequent paranoid state the patient continued to hear "talking through the air." She once spoke of the voices as "some kind of an echo throwed in the room." The content varied from such harmless remarks as "Do you like it here?" to remarks touching on painful complexes; thus the "echo" brought to her a remark about a "black hairy looking person" (she has marked hypertrichosis). These hallucinations occurred almost every day. There were no visual hallucinations.

Mood.—During the delirium the patient's mood fluctuated. During the subsequent paranoid state she was uniformly depressed. She seemed lacking in energy.

The account up to this point was written early in May, 1932, in preparation for the meeting of the Association. The following account represents what happened thereafter. In the summer the patient gained a little in energy output, at times even playing tennis with moderate vigor. Nevertheless, she continued depressed and up to the present (October, 1932) has not yet been straightened out in the matter of the year. The only change in this respect is that in recent months she has generally thought it 1933 (never 1932), whereas formerly she had usually asserted it was 1934. The interview of October 20, 1932, is typical. She stated that in the hospital it is 1932, while outside "it *must* be 1933." I showed her some out-of-town newspapers, dated 1932. She replied, "That's the easiest thing in the world. If I had a printing press, I could print it too. What's more, these papers were not printed in York and Gettysburg: they were printed right here (in the hospital)." (Why do you think so?) "I don't think they'd do any of these tricks in the home towns." (Why would we want to print 1932?) "Just to dunce us—to make us believe that our minds are not working right." (What motive could we have?) "Not a good one. Is it a money-making scheme, or what *is* it?" (How could money be made thereby?) "You print these papers and then sell them around here."

The patient continues to hear "talking through the air." She is convinced that everybody else in the hospital hears it too. She retains only four digits forward and two backward (October 20, 1932). Calculation is poor. The neurological examination shows no abnormalities, save that the pupils are a trifle irregular. Her physique is of the dysplastic type (obesity; hypertrichosis).

Comment.—Inadequate information does not permit us to be certain of what transpired prior to the patient's admission to this hospital. There are two possibilities. Either (1) on approximately July 25, 1931, the patient became delirious by virtue of bromide medication begun prior thereto, or (2) the more severe symptoms which began on July 25 were those of an endogenous psychosis, the delirium not beginning until some time later. In either event, the patient, on her admission to this hospital, was in a delirium which continued five weeks, after which a chronic hallucinatory-paranoid state became evident, associated with weakness of memory.

If the delirium began on July 25 instead of later, another question must be asked. Prior to July 25 the patient had apparently an uncomplicated depression (although our information concerning this period of her illness is meager). The termination of the delirium found her with pronounced paranoid symptoms. We must therefore ask—does bromide intoxication ever modify a pre-existing psychosis, so that the psychotic picture after the termination of the intoxication differs from that before the intoxication? This question will be discussed below.

This case offers a remarkable opportunity to differentiate between delirious disorientation and paranoid disorientation. The delirious individual is disoriented because (in part) of dulling of his perceptions; he answers questions relating to time, place and person unhesitatingly, and his answers to the same question often vary considerably. As a general rule *he does not know that other people regard his answers as incorrect.* On the other hand, in paranoid disorientation, the individual because of delusions refuses to believe the plain evidence of his senses. He sees that the newspapers are dated 1931, but does not believe it is 1931. He sees his mail plainly addressed to him at Harrisburg, but does not believe the place is Harrisburg. He observes that the man who visits him carries a stethoscope, but does not believe him a physician. Consequently he reacts to questions in a characteristic way. The delirious individual answers unhesitatingly (or, if he hesitates somewhat, it is because of fumbling, and not because of suspiciousness). The paranoid individual, on the other hand, meets the question warily and frames the answer circumspectly, wondering all the time at the motives that might exist in the mind of the examiner. He answers that this is *alleged* to be Harrisburg, but says or implies that he doubts it. He may or may not reveal what he regards as the correct answer. If he gives what he believes to be the correct answer, it shows a great tendency to uniformity. Thus, the present patient, Mrs. D., invariably during 1931 gave the year as 1933.

As regards the motive imputed to the examiner, Mrs. D. in the beginning said many times that we were testing her mind by ascertaining whether she was easily deceived. More recently she has said that we were trying to "dunce" her, *i. e.,* trying to make her believe that her mind was not "working right," and that our mo-

tive was anything but good. On the other hand, Mr. B. had no such feeling of certainty about our motive. Following the disappearance of his paranoid disorientation he said (November 18) that during the period of the disorientation he had wondered why we should want to deceive him, but had been unable to give himself a satisfactory answer.

CASE 5.—*Summary. Bromide delirium occurring in an individual worried over high blood-pressure. Twenty-eight days after the discontinuance of bromides the psychotic picture changed: she entered a "chronic delirium" with extreme psychomotor retardation and distressed perplexity.*

Mrs. E., age 51, had always been a healthy, even-tempered woman. In November, 1931, she began to feel weak and learned for the first time that she had high blood-pressure. This worried her. She was given bromides *and put on a salt-free diet.* On January 4, 1932, she was put to bed on account of weakness. The following day she began to have imaginations, said that birds were picking at her frame, etc. She continued to imagine things, and on January 11 was admitted to the Polyclinic Hospital. Dr. Englehart, resident physician at the Polyclinic Hospital, reported that throughout her stay there she was disoriented. She was so drowsy that she needed no sedatives, and received no bromides there. On January 28, 1932, she was transferred to the Harrisburg State Hospital.

On admission here the patient was in a typical delirium. She rambled in her talk, gave the date and year incorrectly, mistook the physician for a clergyman of her acquaintance, and thought she was in Middletown (near her home town). She had auditory and visual hallucinations. There was no rash. The blood-pressure, which had been high previously, was now normal. The first blood examination, on January 29 (*18 days* or more after the discontinuance of bromides), showed 75 mgm. of bromides per 100 c. c.

In addition to the ordinary symptoms of delirium the patient showed a remarkable subjective *disturbance of equilibrium.* On January 28, in describing her condition of the preceding few days, she said, with painful affect, "You feel up so high and down so low. I used to feel as though I was going up and down. It just seemed like an elevator—like I was rocking—like it was going up and down pretty fast." On January 31, when asked what county this is, she replied, "I guess you can't say it's *any* county." She had the sensation of flying. *"I believe we're right over Middletown."* (She was in bed at the moment.) "We might be up in the sky, or somewhere. I feel as though I ought to be able to fly or something. . . . . I'll tell you how it feels: if I unwrap myself (removes bed-covers), I feel loose, just as though I'm going down." (Do you feel as though you're not on solid ground?) Emphatically, "Yes. *It seems as though they can't get it level. One minute it seems as though there's too much on this side, and the next minute there seems to be too much on the other side.*" On February 1 she said, "Everything seemed to be going 'round and 'round. Day and night I thought I was going around. . . . . *It used to seem as though I was on a wire. I felt like*

I was going to fall off." February 15: "It seems like we aren't on a level. Sometimes that side (pointing to the wall at the foot of the bed) seems to be up and this one down—and up and down, and 'way over and across."

Remarkable also were certain highly *uncomfortable somatic feelings.* On January 31 she said, "My eyes feel as though they might be—(gropes for an expression)—well, not exactly empty. And my head—I don't know whether it feels as though there mightn't be any brain in it—as though it might be empty. . . . . I used to be tall when I was young. (Looks at her body.) It *looks* long enough, but—I just feel as though I'm shrinking. (Pointing to umbilicus.) See how it's twisted in there . . . . I feel all drawed together. I feel too narrow. I'd like to be broad enough to walk. I feel as though I have a baby stomach (looking at abdomen)—it feels small. My breasts feel too close together: they feel too small. I just watch those people walk by the door: they all look heavier than me." Her vulva "feels entirely too small. It feels as though there's none there. It just feels as though there's a part of my body that's missing." February 2: "My neck feels too short. (My eyes) feel as though they're a little bit cross-eyed. How does my face look? Does it look big or little? It feels little." February 3: "It always seems as though my head ought to be bigger and have more in it. It feels as though it sort of went together more" (makes a movement compressing the two sides of her head). "My chest is getting smaller again." Her vulva "feels all closed up, as though I wouldn't be able to pass any water. . . . . I feel—just as though I don't have all that belongs to me. My ears ought to feel as though—I feel as though one (left) is bigger than the other." (Do your eyes work alright?) "Oh, they seem to dance around when I close them." February 15: "It feels as though my body isn't put together at *all* right."

The patient continued to show the usual signs of delirium, and (though recognizing as unusual the somatic sensations and the disturbance of equilibrium) I regarded her case as an ordinary bromide delirium and expected her to clear up in the first part of February. What actually happened is that on February 8 her condition underwent a striking change. Formerly she had talked freely and seemingly without effort. She now began to show a remarkable degree of psychomotor embarrassment. It was thereafter not unusual for a fifteen-minute interview to produce but a dozen words from her. *At no time did she show the slightest evidence of negativism.* She always gave one the impression of trying, but finding it difficult or impossible, to do what one asked of her. To seize a pencil was difficult, to write with it was impossible. To express verbally the simplest thought seemed to require a tremendous effort (while pantomimically she expressed herself with facility). When I asked her to repeat numbers after me, she said, "I don't know if I *can.*" When asked to touch my finger, she said, "I'm afraid I can't do it."

Further evidence of psychomotor difficulty was seen in the patient's inability to control her sphincters. While before the transition of February 8 she had on rare occasions wet or soiled herself, on February 9 she began to do both daily. On February 17 she stopped soiling, but continued to wet

herself. On February 18 she was unable to void, and her distended bladder was catheterized. Thereafter she required daily catheterization until April 1, since when she has been able to void every day. During April she resumed occasional bed-wetting.

During the period subsequent to February 8 the patient's *affect* was one that would be aptly described by Hoch's term "distressed perplexity." Her expression was almost always a puzzled frown. "I'm all tangled up. I can't straighten things out. I can't remember things." On February 15, when she spoke of her eyes, I asked whether she had ever seen double; she replied, "I guess that was my trouble—I was turned *this* way too much and then I was turned the *other* way, and I got all tangled up." On March 3 I asked, "Where do you live?" She replied, "I don't know now. They just change it all the time." April 18: (What city is this?) "They *say* it's Harrisburg, (but) I don't know whether it is or not. *I'm lost.* I don't know which way to get out." (Where do you come from?) "I did come from Royalton (correct)—in the beginning—they say it's been so long already." (Don't you as a matter of fact come from Royalton?) "I don't know. They *say* I've never been *out* of here. *Everything's so complicated I don't know how to express my thoughts.*" (You said you *did* come from Royalton. Don't you still come from there?) "I've been in here so long, it seems this *must* have been my home. . . . . I guess you're puzzled about it as much as *I* am, aren't you?" (On several other occasions the patient expressed the belief that people about her were puzzled.)

In connection with her feeling of having been here so long, it is noteworthy that on one other occasion the patient revealed an uncomfortable disturbance of her sense of time. February 2: (How long has your daughter been married?) "That's another thing I can't get away from—*time flying so fast—everything seems so long,* as though it's never going to end. It's hard to judge the time and everything. It seemed like the nurses were so *busy,* you know." (See also an observation made on June 15, 1932, recorded below.)

This painful feeling of an inordinate length of time is somewhat akin to a painful sense of distance revealed to the relatives on March 26. To quote the daughter, "She told us that when we leave it seems to her as though we are a thousand miles away."

The patient recognized her relatives, but at the same time their identity seemed to puzzle her. They reported that when they visited her, it took her a minute before she seemed able to place them. On March 9 she was visited by her daughter and sister-in-law. After they had gone, I asked, "Who came to see you today?" "I don't know *who* they were . . . . I guess it was my daughter and sister-in-law." On April 6 she doubted the identity of her daughter. "I believe they just *tell* me it's her. I don't believe it *is* her." (Doesn't she *look* like your daughter?) "Yes, *but she seems too little.*" April 7: (She really *is* your daughter isn't she?) "Yes, but it don't *seem* like her." On February 15, when asked who I am, she replied, "I don't know—are you a sheriff or something?" Thereafter, to the same question, she answered, "I don't know." On April 6 she told her relatives

that no one seemed real to her. To quote the daughter, "If we ask her who someone is, she will say it looks like this or that person. No one seems real to her. When the doctor comes in and we ask her who it is, she says it *looks* like the doctor, but she doesn't think it *is* he."

The patient occasionally recognized that she was in a "place for crazy people." On other occasions she thought she was in jail, in the almshouse, or at home. She was never able to reply to questions relating to the date, always saying "I don't know" or "I forget." When asked to recite the months, she was unable to recall the name of a single one. (What comes after January?) "I don't know." (Does December come after January?) "I don't know *how* to answer it."

On several occasions following the transition into the "chronic delirium," hallucinations were found. Thus, on April 18: (How old are you?) "I don't know how old I am, but they claim I'm awful old." (Who claims this?) "The voices." (Where do they come from?) "The radio." (What do they say?) "I don't know—I just imagine it, I guess."

There was an air of hopelessness about the patient. April 18: "I feel as though I ought to be dead and in my grave, because nobody can help me." April 23, she said to her daughter. "It's my fault that all these people are here, and they are awfully cross at me. The water is getting nearly all used up; they think I use too much. They are getting tired of me here, and want to get rid of me."

Affectivity was remarkably intact, in the sense that there was nothing resembling the aloofness and indifference seen in schizophrenia. She responded promptly to friendly gestures, and her smile was natural and appropriate. On one occasion only was she peevish. Her relatives reported that on one of their visits in March, when they asked her too many questions, she replied, "Don't ask me so many questions. Everybody asks me questions—I get tired of it."

The account up to this point was written early in May, 1932. In the latter part of May the patient began to show the first indication of emerging from the psychosis. The psychomotor difficulty improved remarkably, so that she was now able to respond to questions and execute requests promptly. On June 9, 1932, for the first time, she was able to write her name. She said, "I can remember things better." (What place is this?) "They say it's the state hospital. Is it?" (Who am I?) "They say you're Dr. Levin. *Are* you Dr. Levin?" (What year is this?) "I always forget. I can't tell you that." (What season is it?) "Spring." (What month?) "May or June; I believe May." (What day?) "Thursday" (correct). She recited the days of the week promptly and correctly.

On June 15, having begun to assist in the ward-kitchen, she said that she found dish-carrying very hard. (You've been in bed a long time; you must learn to do things again.) "Yes—I've been in bed too long, I think that was the trouble." (How long have you been in bed?) "It seems a couple of years. Do you think it *is*?" She retained five and sometimes six digits. She subtracted 7 from 100, but when asked to take 7 from 93, she replied, with embarrassment, "I can't think very well just now—it's funny, I can't remember."

The patient continued to improve and on July 6, 1932, was paroled. Not yet capable of assuming the burden of running her household, she and her husband went to their daughter's home. Up to the present (October, 1932) she has reported twice for check-up. On August 4 she and her daughter reported that she was daily improving. She helped with the sewing and cooking. Her appetite was good. She slept poorly because of nearby railroad trains. She appeared cheerful and normal, save that she seemed to lack vigor and was a little sluggish in thinking. She gave the year correctly, but after several moments' thought. She retained six digits. Neurological examination showed only that the pupils were slightly irregular. On September 24 she again appeared to lack vigor, but was no longer so sluggish in her thinking. She was continuing to improve slowly, but had not yet gone back to her own home.

Comment.—In this case it is known with certainty that before the delirium there was no evidence of a psychosis, save for "reactive" worry over high blood-pressure. While getting bromides and little or no salt, she developed a delirium. About four weeks after the discontinuance of the bromides the picture changed from an ordinary delirium with free rambling talk into a "chronic delirium" characterized by disorientation, painful perplexity and extremely marked psychomotor retardation. After this "chronic delirium" had persisted three and a half months, the patient began slowly to emerge from it. She improved considerably, but four months after the beginning of this emergence she still appeared to lack vigor and was somewhat sluggish in her thinking.

CASE 6.—*Summary. Bromide delirium superimposed on chronic alcoholism and concussion.*

Mr. F., age 55, a pyknic syntonic man, drank hard cider for many years. He was reported to have had several previous attacks of delirium, but we do

not know the dates and the duration. He was said to have taken salty medicine irregularly for several years, chiefly for headache.

On February 18, 1932, at 4 p. m., while unloading a wagon, the patient's hand slipped and he fell backward, striking his head in the right parietal region. He was unconscious until some time soon after midnight. The exact facts of his condition then are not clear, but it seems that for a week or more he showed great dullness and restlessness. He was given large quantities of sedatives. Four weeks after the accident he began to hallucinate, seeing dogs and dead people. He grew difficult to manage, and on April 6, 1932, was admitted to the Harrisburg State Hospital.

On admission the patient showed all the signs of delirium. He had a pronounced papular rash. On April 11 the serum contained 175 mgm. of bromide per 100 c. c. The delirium remained until May 4, 1932, and thereafter the patient was perfectly clear. He now felt well and had no complaints referable to the concussion. His general condition steadily improved, and he was paroled on July 6, 1932. Two months later he returned for follow-up examination. At this time he appeared to be in excellent health, and reported that he was working every day in a warehouse. There was moderate retinal arteriosclerosis.

CASE 7.—*Summary. Bromide delirium superimposed on late epilepsy.*

This case is almost a duplicate of Case 2. Mrs. G., age 63, who had always enjoyed unusually robust health, began at 60 to have typical epileptic seizures, sometimes followed by brief clouded states. She became increasingly irritable and stubborn. She took a great deal of bromides. About January, 1932, she began to imagine things. She was admitted here April 15, 1932, in a typical delirium, which cleared up on April 30.

A striking feature of the case is that, as in Case 2, following the termination of the delirium there remained a slight paranoid aftermath, relating to a woman (Annie) whom the patient had befriended and who during the preceding year had been living in the patient's home as a companion (the patient was a widow and her only child had died). During the delirium the patient expressed to us the belief that Annie was taking advantage of her absence from home to remove belongings of the patient. Even after she became fully oriented and clear she retained this belief. Thus, on May 4 she related that prior to her admission here she had seen Annie going about the house in a white uniform. She was now certain that Annie intended to claim that she had attended the patient professionally as a nurse, and feared that Annie was going to send in a bill for these services. Not until May 9, 1932, did she admit that she might have been doing Annie an injustice in suspecting her.

The serum on April 18 contained 150 mgm. of bromide per 100 c. c. We have information—though not of a positive nature—that the bromides were discontinued ten days before admission. In that case April 18 would be two weeks after discontinuance.

Because of the patient's unusually stubborn and irritable disposition, her relatives (several brothers and a son-in-law) have shown no desire to take

her home, so that she is still in the Hospital (October, 1932). She retains sometimes six and sometimes seven digits. Remote memory has been moderately affected. Calculation is slowly performed. There is marked peripheral and moderate retinal arteriosclerosis. The optic discs are normal. The left pupil is of moderate size, while the right is small; both are circular and react well. There is moderate tremor of the outstretched fingers.

In the hospital the patient had epileptic attacks as follows: April 25, one; April 26, three; June 3, six; September 28, four.

A CASE OF BROMIDE HALLUCINOSIS.

The *sine qua non* of delirium is disorientation. In hallucinosis, on the other hand, the patient hallucinates while fully oriented. Since hallucinosis may in some cases be looked upon as an "incomplete" delirium, I have long wondered whether bromides, like alcohol, could produce it. The evidence today permits us to answer this question affirmatively.

The literature so far contains only two cases that may unquestionably be classified as bromide hallucinosis: Diethelm's Case 3, and Andrews' Case 5. Sippe and Bostock report a series of cases, of which Case 9 sounds as if it *might* represent a case of hallucinosis. Unfortunately these authors do not state whether or not the patient was oriented. I myself saw a score of cases of bromide delirium before I came across a case of hallucinosis. A report of this case follows.

CASE 8.—*Summary. Bromide hallucinosis superimposed on a neurotic picture.*

Miss H., age 20, was all her life "high-strung and nervous." On excitement she trembled, so that "people always asked me whether I was doctoring for my nerves." She was of low average intelligence, completed the seventh grade and worked in factories thereafter. In 1930 her "nervousness" increased. In the summer of 1931 she became so "nervous" that she began to consult physicians, and received much bromide medication. *In November, 1931, she began to see and hear things, although always oriented.* She continued to get large quantities of bromides. She called her physician's attention to the fact that she was getting a rash. He assured her that this was merely from the medicine, and advised her to continue to take it, since she needed it for her nerves. On January 29, 1932, she was admitted to the county almshouse; at this time *bromide medication ceased.* Meanwhile the hallucinosis continued, and she was transferred to the Harrisburg State Hospital on February 15, 1932.

Our observation showed a girl in excellent rapport, with none of the affectivity disturbances of schizophrenia. She was at all times well oriented

in all spheres. She was hallucinated and continued so until the end of February when she made an excellent recovery, with perfect insight.

On admission there was a marked papular rash on the face and chest. The blood was not examined for bromides until March 10. On that day, *six weeks* after the discontinuance of the drug, the serum contained 25 mgm. per 100 c. c.

The following more detailed account of the hallucinosis is compiled from observations made before the patient's recovery, and from her subsequent retrospective account.

The most prominent symptoms were *auditory and visual hallucinations.*

Auditory Hallucinations.—The patient, who had been brought up to believe in the personal reality of God and the devil, heard voices which she at the time assumed to be the voices of God and the devil. The voice of God told her to "do good and live honest." The devil's voice told her to misbehave, fight, curse, run around with men. Specifically it said, "Go out in the world," "Don't waste your time with church. Go out and enjoy yourself." In the hospital the devil's voice carried depressing thoughts: "You'll have to stay here years and years." On the other hand she heard God's voice saying, "Don't worry: you'll get better and you'll be going home again." Both voices seemed to emanate from the lower part of her sternum. Invariably they sounded like a "very quiet whisper."

Visual Hallucinations.—The patient saw elephants, giraffes, horses, lions, cats, black snakes, and the faces of black men. I saw the patient on one of the occasions when she was hallucinating, and have rarely seen such an expression of horror. On a later occasion, referring to the hallucinated animals, she said, "I thought they were coming toward me to bite me." On one occasion she saw a tiny elephant, about three feet high.

Olfactory Hallucinations.—From time to time the patient perceived an intense odor of flowers. Though the odor was fragrant, it disturbed her because it made her think of her dead father: "it was the odor of flowers that you send to a funeral."

"Electricity Feelings."—From time to time she had a painful vibratory feeling, especially in her head, chest, hands and feet. February 23: "I used to think somebody else does (causes) it, but when I don't have it I think different. When I *have* it, I get so wound up I don't know *what* I'm thinking."

Paroxysmal Hypertonia.—The patient had a remarkable symptom consisting of sudden stiffness of the hands, occurring when she was frightened and lasting several seconds. On another occasion I shall consider this symptom.

The course of these symptoms is noteworthy. The auditory, visual and olfactory hallucinations and the "electricity feelings" began at approximately the same time in November, 1931. By the time of her admission to the hospital, the visual hallucinations were already less frequent, and they ceased entirely at the end of her first week here. The auditory hallucinations were quite prominent on admission and cleared up at the end of February. The "electricity feelings" likewise disappeared at the end of February. Only the

olfactory hallucinations continued beyond February. These no longer disturbed her; they occurred at night, when she perceived the odor of flowers while composing herself for sleep. They ceased entirely in the middle of April. The paroxysmal hypertonia, the last symptom to occur, was also the first to disappear: it was first noted at the end of November, 1931, and last noted on February 13, 1932—two days before admission.

While neither the relatives nor we had observed any evidence of disorientation, the patient herself recalled that on two occasions at the almshouse (January 30 and February 12), when visited by her family physician, she did not recognize him. On these occasions she also did not recognize her whereabouts. On each occasion the disorientation lasted several hours, and was followed by a realization of having been disoriented.

The patient was paroled on May 3, 1932. At that time she still had a slight remnant of the rash present on admission. I took this remnant to be a pre-existing acne. However, when I next saw her, on July 1, 1932, the rash had entirely disappeared. She reported that her "nerves" were—as they always had been—"bad," *i. e.*, she was easily upset. She appeared for follow-up examination on October 29, 1932, reporting that her nerves were still "bad." She was eating well and doing her share of the housework. She was at home only because jobs were hard to get. There had been no recurrence of hallucinations, "electricity feelings," or paroxysmal hypertonia. Her behavior was normal, and she was in excellent rapport.

Comment.—In this patient, bromides apparently precipitated a hallucinosis which began in November, 1931. In the same month a pronounced rash appeared. The bromides were continued until the end of January, 1932, and the hallucinosis cleared up at the end of February (save for the olfactory hallucinations, which persisted in a mild form until the middle of April).

On two occasions before admission the patient was for several hours disoriented. It is probable that had her intoxication been more severe or her "powers of resistance" weaker, she would have entered a full-blown delirium.

An extraordinary fact is that these two brief episodes of disorientation occurred *after* the discontinuance of the bromides. The first occurred either the same day or the day after the discontinuance. The second occurred about 12 days later. The significance of an exaggeration of symptoms following discontinuance will be discussed below.

SOME SPECIAL SYMPTOMS OF THE CASES HERE REPORTED.

It is not my purpose to review all of the symptoms of our cases. We shall confine our attention here to certain special symptoms.

A. *Disturbances of Sensation.*—We are concerned here not with hallucinations but with certain other disturbances of sensation reported by our patients.

(1) *Visual disturbances* were prominent in two cases, Mrs. C. and Mrs. E. They occurred in two chief forms: disturbances in the apparent color of objects and disturbances in their apparent size and distinctness.

Disturbances of Color.—Mrs. C. complained once (October 29) that objects a few yards away had a black color: *e. g.,* the nurses had black faces. (Her visual hallucinations, which occurred in great profusion, included colored men and a colored infant.)

Mrs. E. complained frequently that things did not have their right color. Usually things had a black color. Thus, on January 30, at noon, she said, "I guess it's night—or is it daytime? Which is it? I believe my eyes aren't what they ought to be. If I have them shut, everything is just black—if I have them *open,* I mean." On many other occasions she thought it was night when it was really day. The relatives reported that during her stay at the Polyclinic Hospital just prior to her admission here she had complained that people seemed to have black faces. On the other hand, on two occasions she apparently perceived lightness correctly, but it did not tally with her notion that it must be dark, the result being that she complained that things look lighter than they should: (1) February 4, 10 a. m.: "It's dark in here—it just seems as though it's light" (the day was dark and cloudy). (2) February 15, 11 a. m.: "It looks as though it's daylight, but I don't believe it—is it? I believe it's night, and yet it always looks like daylight." Once (February 3) she complained of inability to appreciate other colors: "everything just looks like black and white." (She correctly identified red and blue.)

Disturbance of Apparent Size and Distinctness of Objects.—Mrs. C. complained many times that "things look blurred," "everything is all blurred." On October 26 she complained bitterly of *macropsia:* "What's the matter with me today! Everything looks big. Everything looks enlarged. . . . . Your whole face looks enlarged." I showed her a pencil; she held it and said, "It feels enlarged and looks enlarged."

Mrs. E. had both *macropsia* and *micropsia.* During her two weeks at the Polyclinic Hospital she complained on different occa-

sions that things looked too big and too small. On February 15, watching me write a moderate-sized hand, she said, "Nobody can read that—the letters and figures are so small." Allusion has already been made to her statement on April 6 that the young woman who had visited her "seems too little" to be her daughter. She repeated this statement on a half dozen occasions. Her numerous statements that parts of her body were too small probably represent disturbances partly of somatic sense and partly of visual function.

Mr. F., after his delirium, recalled that while looking at a newspaper during the delirium he had thought that the small print looked much smaller than usual.

Mrs. G. said (April 19) that things looked "blurred."

I shall here interpolate an observation made by a patient studied recently at the Harrisburg State Hospital and not included in the present series of cases. This patient had a bromide delirium superimposed upon early dementia paralytica. In giving a retrospective account of the delirium, she said, "Lots of times my eyes were as though covered with a scum. I could see, but things looked misty. I couldn't see things plainly."

In Case 2, reported by Doane and Weiner, the patient, during a bromide psychosis, complained of transient blindness.

The conclusion is justified that at least some patients with bromide delirium *do not see well.*

It must not be assumed that the visual disturbances here recorded concern only the sensory portion of the visual apparatus. Seeing is in actual fact a *sensorimotor* function. No one has elucidated this better than Hughlings Jackson in his third Croonian lecture.[5] Jackson pointed out that when we see, *e. g.*, a brick, our perception of its color occurs during activity of sensory pathways, while our perception of its size and shape occurs during activity of motor mechanisms (ocular muscles). This is in harmony with the facts noted by Jackson elsewhere[6] that micropsia may occur with paralysis of the internal rectus muscle and in healthy people after the instillation of atropine. It is clear, then, that the visual symptoms with which we are here concerned are referable to disturbed functioning of the visual *sensorimotor arc.* Disturbance of the sensory portion of the arc results in abnormal color-perception; disturbance of the motor portion (more accurately, the eye muscles

and their proprioceptive fibres) results in macropsia, micropsia and blurring of outlines.

It is pertinent to add here that two patients had Lilliputian hallucinations: *Mr. B.* saw "ladies two feet tall" (September 17), and *Miss H.* spoke (February 23) of having seen an elephant three feet high. The hallucinated animals seen by *Mrs. C.* were usually small animals of natural size (squirrels, frogs, etc.).

It is also worthy of note that in two of Schilder's cases of alcoholic hallucinosis microptic and macroptic hallucinations occurred.

(2) *Auditory disturbances,* apart from hallucinations and illusions, were infrequent. *Mrs. E.* alone complained once (January 30), "I don't hear so good." (How does it show?) "The train seems so far away." (Do you see a train?) "I imagine I *hear* them, and the harder I try to hear them, the farther away they go." (The hospital is indeed so situated as to enable one to hear distant trains.)

Mrs. E. also complained of a remarkable auditory and, to some extent, visual *hypersensitiveness.* On January 31 she complained, "It seems like it's going to last forever—this noise. It's like if someone gets real close to you and rubs around your ear" (she illustrated by inserting her finger in her ear and rubbing it around). February 2: "Everything seems *so loud* in here—the telephone bell and things like that." The same day: "I imagined I heard voices in the air—*always so loud and so noisy.* Everything seems so loud here, and I (feel like I would) just like to *stay in the corner in the dark.* The lights seem too bright."

(3) *Olfactory disturbances* occurred in three patients. *Mrs. C.* complained (November 3) that she had smelt "fresh meat burning" during the previous night. *Mrs. E.,* while speaking of other complaints (January 31), added, "And I can't smell very well either." On February 1 she smelt "ether or something." The olfactory hallucinations of *Miss H.* have already been described.

(4) *Gustatory disturbances* occurred in two patients. *Mrs. C.* said (October 20) that the drinking water tasted of lime. *Mrs. E.* said (February 1) that things "don't taste right"; she gave no further details.

(5) *Disturbances of Kinesthetic and Somatic Senses.*—The remarkable disturbances of equilibrium sense and of somatic sense in *Mrs. E.* have already been given in detail. In two other patients

I found slight kinesthetic disturbances. When *Mrs. C.* picked up my pencil (October 26), she said that not only it looked "enlarged" but also *felt* "enlarged." In another case that does not belong to this series (a woman who developed a delirium from large doses of bromide given because of discomfort incident to a fracture), the patient, while in bed, told her daughter, "Take me off the *floor*."

B. *Disturbances of Association.*—Thanks to Bleuler, we know that one of the "basic" symptoms of schizophrenia is a characteristic type of illogical association of ideas. Many concrete examples will be found in Bleuler's monograph on schizophrenia. I have recently reported a number of examples in a series of schizophrenics including several very early cases.

In the present series of cases of bromide psychoses I have found some excellent examples of the illogical associations of schizophrenia. Nine of these will be cited.

(1) *Mr. B.*, on September 28, when questioned about the place, said that he was now in the northeastern part of Philadelphia. (How do you know it's the northeastern part?) "From the way the sun shines into my room." The false logic of this reply is obvious. A house in the southwestern section and another house in the northeastern section might be so placed as to receive the sun's rays at exactly the same angle. The angle at which the sun's rays enter the house at a given moment depends on the direction in which the house is facing. The patient failed correctly to discriminate these ideas, with the result that he made an illogical reply.

(2) *Mr. F.*, on April 16, gave the date as the first week in December. (In other words, Christmas will soon be here?) "No, that's already gone." (In that case it couldn't now be the first week in December.) "Then it must be the *second* week in December." (What day is Christmas?) "December 25."

When the delirium cleared up and I told Mr. F. of this conversation, he smiled instantly at the false reasoning.

These two examples represent the purest kind of disturbance of reasoning.

Confusion of Abstract and Concrete.—A schizophrenic patient, following the subsidence of an acute catatonic episode, was explaining to me what had prompted him one night to walk in a circle around his bed continuously for many minutes. He said, "I felt

that if no one stopped me and I walked on and on and on for a period of a few days, *I would eventually meet my friends on some understanding.*" This patient confused the abstract and figurative concept of meeting his friends on a common basis of understanding with the concrete and literal concept of meeting his friends physically by wiping out the intervening miles. Such confusion of abstract and concrete is an everyday occurrence in schizophrenia. Closely similar observations were made in two of our bromide cases.

(3) *Mrs. E.*, as has already been indicated, suffered from a disturbance of equilibrium sense. She had the uncomfortable sensation commonly felt on going up and down in an elevator, etc. In three remarkable statements she tied up these and other sensations with geographical concepts. (a) On the day of admission, January 28, she said, " It seems like you're *up North* and *down South.*" (By " you " the patient did not mean the second person singular but the impersonal " one.") Asked to explain her statement, she said, " You feel *up so high* and *down so low.* I used to feel as though I was going up and down. I used to feel as though I was going *from the cellar to the garret* all the time." We see here the confusion in the patient's mind of feeling as though one is going up and down and feeling as though one is " up North and down South " (North and South on a map occupying the positions respectively of up and down). (b) The same day, in speaking of the month, she said, " It just seemed to me as if we didn't have any winter at all (the winter had in fact been very mild)—*and that's what made me think we were up North and down South.*" (c) February 1: " I just imagine so many things. . . . ." (What are some of them?) " That I went so far *around the world.* I imagined I was *going around,* day and night I thought I was going around, *everything seemed to be going 'round and 'round.*" Here we see confusion of " going *'round and 'round* " in the sense of giddy twirling, and going " so far *around* the world."

(4) *Mrs. A.*, on July 17, said that her memory had lately been bad. (How did you notice it?) " I noticed *flying ideas* got in my mind. I was sitting at the table and it was *just like birds flew past me—shadows flew by and they looked like birds.*" We note here the interesting juxtaposition of flying (fleeting) *ideas* and flying *shadows.*

False Interpretations.—We are not concerned here with illusions.

(5) *Mrs. D.,* in speaking of "talking through the air," by means of which she heard whispers from far away, said (October 21): "I don't understand it myself. The only explanation for it that I would know would be that *the world is getting weaker.*" What interests us here is the structure, rather than the falsity, of her explanation. She was puzzled by a new experience, *i. e.,* hearing "sounds" never before perceptible to her, and she accounted for it on the basis of a weakening of the conducting medium. I presume that in her mind the matter was comparable to the transmissibility of vibrations on a bridge: a pedestrian on one end of a bridge will feel the vibrations produced by an ox-cart on the other end if the bridge is frail, not if the bridge is massive.

(6) *Mrs. E.,* said (February 15) that she was "blind." (You see me, don't you?) "Yes, *but with one eye*—or two, maybe." We see here that in accounting for her visual impairment, her first impulse was to assume that she saw with one, instead of two eyes.

(7) In my many conversations with *Mrs. E.,* I was often perplexed by her statements. She saw this and assumed that her perplexity and mine were of the same type. From many examples I select the following. I have already cited the instance of April 18, in which I strove to get her to elucidate some puzzling remark, whereupon she said, in no spirit of jest, "I guess *you're* puzzled about it as much as *I* am, aren't you?" On February 15, when after a long interview she seemed tired and I asked, "Shall I go?" she replied, in a tone of helpless uncertainty, "I don't know *where* to tell you to go"—as though uncertainty of destination had prompted my question.

Mrs. E. imputed perplexity not only to me but also to everyone else. She was on a busy ward, her room so situated that she saw many nurses passing by and frequently heard the unlocking of the front door. February 1: "It seems like they (the nurses) don't know which way they want to go out there. They circle 'round and 'round and then they stop and then they go again." February 3: "They're having so much trouble opening the doors this morning, you're liable *not* to get *out.*" February 15: "I guess we're *all* blind in here, that's the trouble."

Miscellaneous Instances.—

(8) *Mrs. E.,* when asked the date (February 15), replied, "I believe, the way I hear them talk, it's Easter, isn't it?" (What

did you hear?) "About eggs and things." (What did you hear them say about eggs?) "Fried and soft-boiled." We deal here with what Bleuler calls "mediate association."

(9) *Mrs. E.*, looking out the window (February 2), exclaimed, in a tone of great surprise, "Why there's snow outside! (correct). Then it's *cold* out!" (Are you surprised?) "Well, it seemed *warm* in *here*" (puzzled). She was unable to solve this seeming discrepancy, and when I finally said there was heat in the building, she replied, "I suppose there is."

In view of the fact that the association disturbances commonly met with in schizophrenia occurred with such relative frequency in our bromide cases, it is the more noteworthy that the *affectivity* disturbances of schizophrenia (aloofness, apathy, etc.) were in seven of our eight cases unquestionably absent. Only in the case of *Mrs. D.*, whose reaction approaches closely the schizophrenic, there occurred at times a certain apparent indifference (which, however, could well have been depressive hopelessness).

C. *Mood.*—Naturally the mood of a delirious patient is rarely the same from one hour to the next, yet nearly every patient has a characteristic predominating mood. *Mrs. A.* was cheerful, with some periods of irritability. *Mr. B.* was depressed; he believed his wife had become interested in another man. At times he was afraid, thought he was in front of a hidden machine gun, etc. *Mrs. C.* was generally in great fear, ascribing this to the many loathsome animals she saw. *Mrs. D.* was invariably depressed, as was also *Mrs. E. Mr. F.* was invariably euphoric. Although profoundly disoriented in all spheres, he "never felt better in my life." He had the extravagant buoyancy of an alcoholically intoxicated syntonic man. *Mrs. G.* was always talkative and cheerful. *Miss H.* was cheerful, save when hallucinating, at which times she was often terror-stricken.

In four cases (A, B, C, and E) instances of jocularity were observed.

D. *Insight.*—Delirious individuals are so out of touch with the situation that one might think they would rarely, if ever, have good insight. The contrary is in fact the case. In our series of cases, only *Mr. F.* and *Mrs. G.* at no time had insight. In the case of *Mrs. A.* satisfactory data on this point are not available. In the remaining five cases insight was at times present, sometimes

to an astonishing degree. To be sure, in no case was insight complete. The clearest insight was shown by *Mrs. C.*

Mrs. C. often spoke of being "confused" and "mixed up." She had a disagreeable "conglamered" head feeling; *e. g.*, on October 22 she said: "My head is all conglamered up. I can tell you I am not the same as I always was. I am not that clear—(gropes)—that snappy step—you know how you are when you're well. You know when you ain't got that pep and that snap." (Are you able to think well?) "Oh, no, that clearness is all gone." On the same day, pointing apprehensively, she said: "Can you see that snake wriggling its head over there? *I know it's not real* but it worries me just the same."

THE PATHOGENESIS OF DELIRIUM.

Because during the mental examination it requires but a few simple questions to ascertain that the patient is disoriented, the psychiatrist is apt to look upon disorientation as a simple phenomenon. A moment's thought, however, will convince one that disorientation is an exceedingly complex phenomenon. Let us here consider its components.

In so far as we may generalize from only seven cases, the following two statements seem justified.

(1) At least some patients with bromide delirium do not see well. Be it remembered that we are not concerned here with hallucinations, but rather with the fact that objects appear to have incorrect colors, or blurred outlines, or they appear too small or too large. Moreover the evidence, though less cogent, suggests that some patients do not hear, smell or taste well, and that they have only imperfect use of their kinesthetic, somatic and equilibratory senses.

(2) A majority of patients with bromide delirium (even when they are not schizophrenics with superimposed delirium) show, at one time or another, association disturbances of the type commonly seen in schizophrenia.

These observations tend to support the conclusion that correct orientation presupposes (1) correct perception and (2) correct identification. The latter, in turn, presupposes correct association of ideas. The importance of correct association should not be underestimated. Conceivably, a patient may correctly perceive every detail of his environment, yet through faulty "putting two and

two together" may utterly fail to identify his surroundings properly. No better demonstration of this is needed than the incident already recorded in the case of *Mr. B.* The patient doubtlessly perceived correctly the angle at which the sun's rays entered the room, but he made therefrom a false deduction respecting the location of the building.

The statement that disorientation rests in part on faulty association expresses a psychological disturbance. Hughlings Jackson emphasized that every psychological disturbance is but the concomitant of a physiological disturbance. In his article on aphasia* Jackson with his characteristic clarity expounded the physiological disturbance correlative with disorientation. In Jackson's terminology, disorientation is a "positive symptom," representing the action of healthy reflex arcs unchecked because of the exhaustion of higher regulative arcs. He considers the case of a delirious man who mistakes the nurse for his wife, *i. e.,* a strange person for a familiar person.* Ordinarily the "well organized symbol-image" of woman is represented by the wife. Seeing the nurse rouses this organized symbol-image. *Recognizing her as a nurse* involves the additional rousing of higher, less well organized, nervous pathways. The latter, being less well organized and therefore less stable, are more easily exhausted. When they are exhausted, the *then* highest pathways will alone be roused by the presence of the nurse, whom the patient will therefore identify as his wife.

Jackson's statement (the exhaustion of higher pathways permits the unrestrained activity of intact lower pathways) describes a physiological event. The concomitant psychological event may be described thus: the failure of the highest associative processes hinders the bringing of the requisite associations to bear on a given object (or situation), and thus hinders the correct identification of that object.

The idea that a complex symptom like disorientation may be composed of simpler "ingredients" is of course far from new in psychopathology. Similar examples could be given, especially in schizophrenia.

*Since reading Jackson's views on this point, I have paid particular attention to the type of disorientation for person in cases of delirium. With astonishing uniformity the mistake is in taking an unfamiliar person for a familiar person. I recall only one instance in which a close relative was taken for a stranger.

BROMIDE PSYCHOSES: CLINICAL TYPES.

If we exclude the so-called "simple bromide intoxication" (characterized by mental sluggishness), we may with certainty speak of two clinical types of bromide psychosis—delirium and hallucinosis. Of these, the former is much the commoner.

It is well known that when a delirious patient once begins to be clear, he is for a while apt to have brief relapses, which in the course of time become briefer and less frequent and finally cease. These relapses may consist of periods of hallucinosis. Thus I recall a case of bromide delirium (not belonging to this series) in which during the relapses the patient, *while fully oriented*, heard voices saying he would be killed, smelt ether, and had other hallucinations. Such hallucinatory relapses remind one of those cases of delirium tremens which merge into a hallucinosis, but with this difference: when delirium tremens merges into a hallucinosis, there is generally no intervening period of normality—the patient, say on the fifth day of his illness, becomes oriented but still hallucinates. In bromide delirium I have never seen such merging; the patient is in a normal mental state for a day or more, and then the hallucinatory relapse may occur.

Of bromide hallucinosis *without* a preceding delirium, three unquestionable cases are known: Diethelm's Case 3, Andrews' Case 5, and my Case 8. In Diethelm's case the hallucinosis began five days after the discontinuance of bromides and lasted several days. In Andrews' case it came on during medication and lasted one week after discontinuance. In my case it came on between two and three months before discontinuance and cleared up one month after discontinuance.

Bleuler believes that alcoholic hallucinosis occurs predominantly in persons with manifest or latent schizophrenia. Whether this is equally true of bromide hallucinosis we cannot now say.

Several of my cases raise the question of the possibility of a third bromide reaction-type. In the case of *Mr. B.* the delirium merged into a hallucinatory-paranoid aftermath which lasted from the middle of October to the end of December. In *Mrs. G.* the paranoid symptoms of the delirium persisted a week or two after she had become well oriented. Finally, in the case of *Mrs. D.* there is a possibility (unfortunately the available data are indefinite) that the patient began with a simple depression, then had a super-

imposed bromide delirium, which merged into a chronic hallucinatory-paranoid state.

These cases make one wonder whether a *paranoid episode,* with or without hallucinations, can occur as a symptom of bromide intoxication. One sees not infrequently patients with paranoid episodes who come into the hospital and clear up a month or six weeks later. It may be that some of these cases develop on a bromide basis, but at the present time we cannot say with certainty.

Is there a "post-intoxication psychosis"? Anything that subjects the organism to severe strain may "precipitate" a psychosis which previously had remained "latent." *E.g.,* psychoses have been precipitated by influenza and other infectious diseases, severe hemorrhage, parturition, psychic trauma, etc. If influenza—a toxic process—can activate a latent psychotic tendency, can bromide intoxication do likewise? In other words, besides the acute psychoses occurring *during* bromide intoxication, are there more chronic psychoses *precipitated by, and continuing long after,* the intoxication? There seems no reason to doubt that this may happen. The case of *Mrs. E.* may be an instance of this, since there is reliable evidence that prior to the delirium she was mentally healthy, save for worry over recently discovered hypertension. The case of *Mr. B.* may be another instance: here we have a hallucinatory-paranoid state which continued an appreciable length of time after the intoxication proper had come to an end. For the present, however, the question of a "post-intoxication psychosis" must remain without a positive answer.

Obviously there is no question but that bromide intoxication may produce a *temporary* change in an already psychotic patient. Thus an epileptic or a hebephrenic may have a bromide intoxication with delirium, upon the termination of which the original psychotic picture is resumed. In Vessie's Case 6, on several occasions intentional bromide intoxication produced a temporary manic state in a patient with severe depression.

We must now ask, can bromide intoxication produce a *lasting* change in an already psychotic patient? It is possible that this happened in the case of *Mrs. D.* Of course, no one believes that the manic-depressive *process* can be converted into the paranoid or schizophrenic *process.* On the other hand, it seems credible that, *e.g.,* a schizophrenia may at first manifest itself in a relatively

simple depressive picture, and that a supervening bromide intoxication may evoke subsequently a more clearly schizophrenic picture.

Wright has reported cases in which bromide intoxication produced, or helped to produce, the improvement or cure of a pre-existing psychosis.

ETIOLOGY OF BROMIDE PSYCHOSES.

Since not everyone with a high concentration of bromide in the blood is delirious, a constitutional predisposition is probably necessary. Probably also the resistance to delirium varies with age and general condition. Cerebral arteriosclerosis, neurosyphilis, chronic alcoholism, etc., probably lower the resistance.

The chance of developing a delirium doubtlessly varies directly with the bromide concentration in the blood. This in turn varies directly with the amount of bromide intake and inversely with the amount of chloride intake.

How long a bromide delirium can last, we do not know. It can probably last six months or more, provided bromide medication is continued. The duration *following discontinuance* is usually from two to six weeks. This duration is determined by the following factors. (1) Organic brain disease undoubtedly retards recovery. (2) Pre-existing psychoses probably have the same effect. (3) Even in the absence of organic brain disease and pre-existing psychoses, there are probably constitutional differences in the speed with which the delirium clears up. (4) The duration after discontinuance probably varies with the duration before discontinuance. In other words, a delirium which had existed several months before discontinuance will probably take longer to clear up than one which had existed only a few days. (5) The administration of salt probably hastens the clearing up of the delirium.

The *type* of bromide psychosis—whether delirium or hallucinosis (or paranoid state, if there be such a type)—probably depends, at least in part, on constitutional factors. It may conceivably depend too on the severity of the intoxication. Thus, in a patient with bromide hallucinosis it is conceivable that sufficiently increasing the intoxication may in some cases cause the hallucinosis to merge into a delirium. As yet, no actual case of this sort has been reported. In several of my own cases I have been led

to believe, in taking the history from relatives, that a state of hallucinosis existed a week or more before the appearance of disorientation, but in no case has this point been observed with sufficient accuracy to permit of a positive statement.

Is There a Withdrawal Delirium?—Several cases have been reported in which the delirium began several days *after* the discontinuance of bromides, and on this basis it has been intimated that the sudden withdrawal of the drug may precipitate delirium. Bearing in mind this so-called "withdrawal delirium," some authors go so far as to withdraw the bromides gradually. Such reasoning, in my opinion, contains a fallacy.

The occurrence of delirium is determined by the concentration of the drug in the blood *and* by the patient's resistance. Therefore, if bromides are stopped on Monday and a delirium begins on Wednesday, it does not necessarily follow that the withdrawal precipitated the delirium; something may have intervened on Tuesday to undermine the patient's resistance, rendering him less capable of remaining mentally clear in the face of a bromide concentration of given magnitude. I do not doubt that there are cases (although, to be sure, none has yet been reported) in which, *e. g.,* a mild influenza or other infection precipitates a delirium in a bromide-loaded person who has already stopped getting bromides and who, had his resistance not been lowered by the infection, might have successfully withstood the intoxication.

A case in point is that of *Miss H.,* who stopped taking bromides when she entered the almshouse and who on two subsequent occasions (one day and fourteen days after her entry) was disoriented for a few hours. Instead of ascribing these episodes of disorientation to the withdrawal of the drug, I should prefer to look for factors that had diminished her resistance. Possibly, had her resistance been lowered sufficiently, she would have entered a full-blown delirium, instead of remaining disoriented but a few hours; in that case it would have been fallacious to assume that the delirium was necessarily caused by the withdrawal of the drug. The same fallacy is seen in a widely prevalent misconception relating to the etiology of alcoholic delirium.

Another objection arises to the hypothesis of withdrawal delirium. Delirium is the visible manifestation of a certain kind of damage to the brain. In the ordinary case of bromide intoxication this

damage is continuous and, as the bromide concentration mounts higher and higher, progressive. Finally the damage reaches such a point of severity as to cause a delirium to appear. Let us call this point the point of crucial damage. Now we can ascertain with reasonable accuracy the day on which the delirium begins to appear, but we have no way of knowing on just what day the crucial damage has occurred. When a delirium begins on Saturday afternoon, shall we suppose that the crucial damage occurred on Saturday morning? This would imply that the discontinuance of bromides on Friday would have averted the delirium. I for one find it difficult to believe that the crucial damage is so closely followed by the onset of the delirium. It seems to me more credible that the relation of the onset to the crucial damage is similar to that of ecchymosis to trauma. I am struck a series of sharp blows on the arm; several days later ecchymosis appears. It would be absurd to say that the ecchymosis occurs because of the discontinuance of the blows; it occurs *in spite* of this discontinuance. Similarly, in bromide delirium beginning after discontinuance, we must not ignore the possibility that the delirium has already been predestined and that it occurs in spite of discontinuance.

The same situation is met with in other poisonings. Thus, industrial physicians know that sometimes the clinical manifestations of lead poisoning make their first appearance *after* the cessation of exposure to lead ("latent" or "delayed" plumbism, as it is commonly called). In these cases no one assumes that the cessation of exposure is a causative factor. On the contrary, it is taken for granted that the morbid process has already gone so far that the mere cessation of exposure no longer suffices to ward off the symptoms.

A third objection to the hypothesis of withdrawal delirium. We know nothing of the degree and duration of intoxication necessary to produce delirium. Let us however assume, for the sake of discussion, that in a given person the bromide concentration in the serum must be at a level of 150 or more milligrams per 100 c. c. for exactly four weeks in order to precipitate a delirium. Let us now imagine that this person is given large quantities of bromides throughout January. On February 1 the bromide concentration in the serum reaches 150 mgm. Large doses continue to be given, and the concentration in the blood mounts steadily. On February 24

the concentration reaches 300 mgm., and on that day bromide medication is discontinued. The concentration now begins to decrease, but on February 28 it is still much above 150 mgm. In accordance with our premise, the patient will become delirious on February 28, *in spite of* the discontinuance of bromides on February 24.

In this discussion of withdrawal delirium I of course do not assert that the considerations adduced by me represent established fact. I contend only that they render it impermissible to regard cases beginning after withdrawal as *prima facie* evidence of the harmfulness of withdrawal.

SUMMARY.

Seven cases of bromide delirium and one case of bromide hallucinosis are presented. In addition to the commonly known symptoms of delirium, the patients in the delirious group showed to a noteworthy extent two disturbances. (1) Disturbances of sensation (referring not to hallucinations, but to subjective difficulty in recognizing colors, macropsia, micropsia, etc.). (2) Disturbances of the association of ideas of the type commonly seen in schizophrenia. It is suggested that these two disturbances are among the "ingredients" of disorientation. The delirious individual misidentifies objects and situations partly because he does not perceive well and partly because, owing to faulty association of ideas, he improperly discriminates that which he has perceived.

Consideration is given to the following topics:

(1) How many clinical types of bromide psychosis are known to exist? We may say with certainty that in addition to "simple bromide intoxication" there are delirium and hallucinosis. There seems to be a possibility that bromide intoxication may also manifest itself as a paranoid state. In two of my cases (Cases 2 and 7) the delirium merged into a transitory paranoid state. In neither of these cases was there evidence of a paranoid state prior to the delirium. In another case (Case 4) the delirium merged into a paranoid state which, at the present writing, has been in existence over 11 months without yet having begun to subside; in this case the patient had a major psychosis before the onset of the delirium, and the existence of paranoid symptoms prior to the delirium has not been ruled out. As far as I know, no case has been reported in which a

paranoid state supervened during bromide intoxication without a previous delirium.

The distinction between " paranoid disorientation " and delirious disorientation is demonstrated.

(2) Is there a " post-intoxication psychosis "? The possibility of this is suggested by Case 5, in which a woman, previously non-psychotic, developed a bromide delirium which, four weeks after the discontinuance of bromides, merged into a chronic delirium with marked psychomotor embarrassment. It seems possible that under certain conditions bromide intoxication (like any other toxic process) may precipitate a chronic psychosis which remains long after the intoxication has subsided.

(3) The etiological factors of bromide psychoses are reviewed. Attention is called to several objections to the recently proposed hypothesis of the occurrence of " withdrawal delirium."

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DISCUSSION.

DR. EMMET F. HOCTOR (Farmington, Mo.).—I feel that Dr. Levin has given us a real contribution in his paper. We know that there is a widespread use of bromides. My own experience with bromide delirium has been limited to three cases admitted to the Farmington State Hospital. There were sensory and association disturbances with disorientation. The factors of individual susceptibility that Dr. Levin has brought out are, I feel, very important and make us realize that patients that are under bromide therapy must be carefully watched if treatment is to be of greatest benefit to them.

The symptoms that Dr. Levin mentioned as characteristic of bromide delirium are quite similar to the symptoms of other conditions, and emphasize the importance of accurate history and study of the patient if we are to properly diagnose the condition as presented on entrance to the hospital.

Of the three cases observed at the Farmington State Hospital, one was due to bromidia, one to nervine and one to triple bromides. The duration of medication had been from two weeks to six months. I feel that in light of the fact that under the direction of the physician or without, bromide therapy is quite universally practiced, Dr. Levin's paper should be a stimulus to us to more carefully note the patients with a history of bromide therapy, that enter the hospitals; attempt to obtain an accurate history of the bromide medication previously given, and also to observe more closely the disturbances in the mental processes which we find.

CHAIRMAN MAY.—Dr. Levin has made such an unusually valuable contribution to the subject of bromide delirium that I am going to take the liberty of asking a question. We have had something like a half dozen cases, I should say, of bromide delirium, due to the use of bromo-seltzer which is supposed to be a perfectly harmless drug and which is used generally by the public and is sold to everybody and anybody. I wonder if he has any experience with that or not?

DR. DAN S. RENNER (Skillman, N. J.).—I want to ask Dr. Levin a question about his bromide therapy and bromide intoxication and delusions, that has been a thorn in my flesh for some 20 years or longer. He gave us a very nice picture of patients that come to us under bromides; but I never have had the courage—perhaps he has—to deliberately try to produce these bromide intoxications in a patient who was not already having some of the symptoms that bromide is given for. We give bromides over a period of time, we get fairly good results, then either we forget that we have gotten the results that we were looking for and we continue to give the drug, or we think that we will give bromides a little longer to be sure of ourselves. The first thing you know, our patient is continuing to react in the same manner that he did before we began the bromides and possibly the reaction is a little bit stronger.

In other words, the bromide intoxication seems first of all to have a sedative effect and arrests the condition that we want to arrest and then in a little while it either increases it or at least keeps it going along in the same style as before. Then we get our bromide rashes, etc. After awhile someone comes along and decides that we should stop our bromides and that particular incident clears up.

I have a great many patients that I have handled myself, or that some of the staff have handled for 10 or 15 years. Once in awhile I forget to tell a new man that a certain patient responds very nicely to bromides for a week but no longer. Later I may go around and see this person in one of these horrible maniacal delusional states. I will suddenly remember that there is a new man taking care of that woman and I will say, "How long has Mary been on bromides?" He will tell me that possibly for three weeks. Then I will suggest that it might be advisable to shift Mary to something else, sodium bicarbonate or whatever I think she will respond to. After that Mary clears up nicely.

I do not know if my experience is unique in that or not. As I say, I never have deliberately taken a case in the quiescent stage and tried to saturate it with bromide to see what could be gotten. I do not know if Dr. Levin could do that. If somebody could get up nerve enough to do that with a few normal people it might mean something.

To me Dr. Levin's cases do not mean that he has a bromide delirium because he has a patient saturated with bromide. We have any number of cases that are supersaturated with bromides and they do not give us any of these reactions. But, we may get one to which we give bromides for the mental disturbance, and we usually get just the story that he has given us.

DR. ALFRED GORDON (Philadelphia).—Dr. Levin gave his experience of intramural administration of bromide in large numbers, but how about the extramural work? I have had very extensive experience with bromides in clinics and private work, but not in institutions. I have been prescribing it for full 20 years and quite extensively, and I have yet to see a single case of delirium.

What I do see is this, that if an agitated patient is kept in a private sanitarium and is treated with bromides for the purpose of obtaining a sedative reaction, but if after the latter has been obtained, you inadvertently continue the bromides, you may have depression as soon as the bromide is cut out and in a day or two the depression disappears. Then you may go to the bromide again if it is indicated.

My method of prescribing bromides is only as a sort of symptomatic treatment in the cases of agitation and restlessness and nervousness. This is applicable to psychotic patients as well as to psychoneurotic patients. If I find any skin complications I shift to another drug or another sedative and then I go back to bromides. But I have yet to see a case of pure bromide delirium or hallucinations or confusion in the extramural practices extending over fully 20 years with continuous work going on.

Dr. Levin brought out a very important point which, in my judgment, was somewhat of a solution but from a different angle than mine. That is the personality problem. But that is the same as with any other drug. There are some people who cannot stand certain drugs. Take aspirin for instance. A patient, of mine, vigorous, six feet in height, could not take aspirin in amounts greater than five grains a day. This is a common experience and I do not have to dwell upon it. It may be the same thing with any other drug. We have to individualize the drug therapeutics.

I watch all such cases very closely. If I have a purely psychotic case I give a small dose of bromide frequently repeated, not in tremendously large doses but according to the age of the individual and at intervals best suited to that individual. I saturate the system with bromide and obtain the effect desired and then I stop. There are some cases where I have to give ten grains every hour. The agitation may be so great that for a period of five or six days in succession this has to be done. Yet, I have never obtained any delirium. Very likely these are the select cases which have no special predisposition for that particular drug and can stand it.

In general therapeutics we have to individualize the cases. There are undoubtedly cases such as Dr. Levin has reported and his report is to be accepted as correct. Some patients cannot stand a prolonged bromide administration and they develop the hallucinations with the delirium.

Judging from the majority of the cases, the method adopted should be a careful one and watched very closely and repeatedly.

CHAIRMAN MAY.—I can assure Dr. Gordon that it is not an infrequent thing at all for us to receive patients brought in by general practitioners because the patients are insane. Their blood is full of bromides and as soon as they get over their delirium they are perfectly well.

DR. DAN S. RENNER (Skillman, N. J.).—What Dr. Gordon reports doubtless holds good in his own experience; evidently he watches his patients. He has them under control and when they reach the danger stage he takes away the bromide and the condition becomes quiescent.

Twenty-five years ago I was in Missouri and there they used bromides by the bottle in a certain section. It wasn't prescribed by a doctor but it was self-prescribed. Some of the people in that section, particularly the alcoholics, had a store of that bromide. They would sober up on bromides and then continue using them and first thing you knew they were insane from the bromide.

In most of the cases that I have had this condition has been caused by self-medication. The cases of bromide delirium that have come to us have been cases that have not been under the observation of the physician. It is true that there are some physicians who prescribe the bromides; but these patients then continue to take the drug without consulting the doctor. They do that with every prescription that they get, although sometimes prescriptions are given that are not to be filled more than once, yet they manage to get them refilled in some way.

If you give enough bromides to any patient you will get bromide delirium, or some symptom of supersaturation of bromides, regardless of the individuality of the patient.

DR. JULIAN M. WOLFSHON (San Francisco, Calif.).—The thing that impressed me most in Dr. Levin's paper is the fact that he has outlined certain symptoms which might call attention during the examination to the possibility that the patient might be suffering from a bromide delirium. Patients come to hospitals suffering from all kinds of psychoses and present various combined reactions; it is important to recognize not only essential psychotic states, but also modifications or added symptoms due to medication. I think that is the significant thing that Dr. Levin has given us today.

DR. E. STANLEY ABBOT (Boston, Mass.).—In my earliest psychiatric days there was the tradition of patients with acute delirious mania coming in and invariably dying. Most of those had been given bromides because of some nervous condition, delirium or some other reason or because they complained of nervousness and didn't sleep. In those earlier days it wasn't recognized that bromide intoxication was a cause of the acute delirious mania as it was then called.

Then back in the seventies and eighties it began to be seen that perhaps intoxication by bromide might be a cause, and so the hospital where I got my early training began stopping bromides at once. Some of those that had taken the bromides were dull and stupid and didn't get enough water to drink and not enough to eat, so they were tube-fed. We began thus to dilute them somewhat and many of them recovered.

At a large municipal hospital where I served for about four years in an executive position, I have seen patients killed one after another by bromides prescribed by the general practitioner, physician or surgeon. The executive officer didn't have sufficient influence with the staff to stop it. As soon as the patient began to show any delirium, whether from pneumonia, typhoid, rheumatism or any other cause, bromides were prescribed. That was always the one prescription that all text books on general medicine gave for any nervous condition. But, those things are changed, or changing.

CHAIRMAN MAY.—The practice still goes on. There isn't one general practitioner, apparently, in a hundred that ever learned this lesson. They have a feeling that it is an absolutely harmless drug and if you cannot think of anything else to give just give a bromide; because if it won't do any good it certainly won't do any harm, and you never can tell—it may do some good. That is the idea.

Is there any other discussion? Dr. Levin, do you wish to say something?

DR. MAX LEVIN (Harrisburg, Pa.).—In our series there was one case that developed after the use of bromo-seltzer—case 1, that of a woman with beginning senility, whose delirium cleared up during the third week after

the drug had been discontinued. She had the cyanosis seen in bromo-seltzer poisoning. A careful examination after the delirium subsided showed a perfectly normal woman, except that she had a slight memory defect.

I have never had occasion deliberately to induce a bromide intoxication. Dr. Wright, at the 1926 meeting of this Association, reported the intentional administration of high doses of bromide for the purpose of improving the condition in manic-depressive and schizophrenic patients.

Dr. Gordon's question has been answered by Dr. Renner. You can give bromides and do it safely provided you watch the patient. What happens so often is that a patient will be going to three or four different doctors at the one time, and each of them will give bromides. I have seen cases where each of the doctors was ignorant of the total amount of bromides the patient was getting.

Another factor is the amount of the salt intake. It is amazing to learn that normal individuals vary in the quantity of salt taken daily, from one gram to fifteen grams, depending on the taste of the individual. If you give 30 grains of sodium bromide three times a day to a patient who gets 15 grams of salt, that will probably not harm him, but if you give the same dose to an individual who takes almost no salt he will be much more apt to develop intoxication.

With regard to Dr. Abbot's question, I do not doubt that many cases of what we used to call delirious mania were really cases of bromide intoxication.

INTERRELATIONS BETWEEN PSYCHOANALYSIS AND THE EXPERIMENTAL WORK OF PAVLOV.*

By THOMAS M. FRENCH, M. D., Chicago.

In the past 40 years there have grown up in neighboring countries two rather large bodies of scientific literature—both devoted to the task of clarifying the mechanisms of behavior and both characterized by a courageous determination to observe facts quite without reference to former preconceptions. Due to the relative inaccessibility of Russian literature, the finer details of the work of the Pavlov school did indeed remain considerably isolated from the scientific thought of other countries; and the psychoanalytic literature, due to the general unpalatability of its findings, has unfortunately been compelled to develop as a sort of foreign body in scientific thought. Nevertheless it seems somewhat surprising that these two bodies of scientific thought have so little apparent relation to each other.

This fact becomes more understandable, however, once we begin to study the work of the two schools more closely; for both in method and in the character of the material chosen for study, the two disciplines stand at almost opposite poles of scientific approach.

The psychoanalytic method, as its name implies, is an analytic one. It starts with a recognition of the complexity of human behavior and contents itself at first with the discovery of the central trends of a patient's behavior, only step by step pushing the analysis from the surface downward. When correctly done it never loses sight of the patient's total situation in the attempt to analyze details—the analysis of details always retaining its place as a part of a more comprehensive insight into the patient's whole psychic situation.

The studies of Pavlov's school, on the other hand, are synthetic in method. The approach is frankly physiological, with emphasis upon a single detail of the organism's reaction, such as the animal's salivary reflexes. It starts with the conditioned reflex as an ele-

*Read at the eighty-eighth annual meeting of the American Psychiatric Association, Philadelphia, Pa., May 30–June 3, 1932.

mentary form of reaction, and in studying this elementary reaction under more and more complex conditions the obvious goal is to reconstruct the complexity of behavior from its elements—much as an organic chemist seeks to produce a complex organic compound synthetically from simpler and better known substances. One feels convinced from occasional comments in Pavlov's accounts of his experiments that he never does really lose sight of the total behavior of the animal upon which he is experimenting, but the emphasis is placed in every case upon the particular conditioned reflex that is under discussion.

There is also a fundamental difference between the two disciplines with regard to the type of material that they select for study. Psychoanalysis has concerned itself primarily with wishes, impulses and drives—that is, with tendencies arising within the organism. Thus sex, hunger, anger, fear and their interrelations and derivatives constitute the principal subject matter of psychoanalytic studies, and the effect of external experience upon the life of the individual is studied for the most part with reference to its influence in transforming and giving new direction to tendencies arising from within.

This fundamental biological point of view is of course never really lost sight of in Pavlov's experiments; but here also the emphasis is different in Pavlov's work. The unconditioned reflexes with which Pavlov's works for the most part are the alimentary and the defense salivary reflexes, but in studying the alimentary salivary reflex, for example, the fact that the animal is hungry is usually taken for granted as a necessary condition of the experiment. Relatively few experiments are directed to problems concerning the interactions of unconditioned reflexes with each other and there is only an occasional comment as to the effects of variations in the intensity of the animal's hunger upon the experimental results.

In view of such wide divergences both in method and in phenomena studied, it is therefore not surprising that the results of the two types of investigation at first seem to have so little to do with each other. If one be convinced nevertheless that the results of these two very extensive investigations must be fundamentally very closely related to each other in spite of differences in emphasis, one must be prepared to make allowances for the differences of

approach that have been discussed. One would expect in the first place to find the closest relations to psychoanalytic findings in the more complex of the phenomena studied by Pavlov, and in the second place one should be prepared to make the most of such hints as Pavlov gives us as to the effects of variations in the intensity of the drives and of interactions between two or more unconditioned reflexes. One might expect also to gain some light from psychoanalytic data as to just how far conclusions reached from the study of salivary reflexes can be generalized to apply also to other aspects of behavior.

One other preliminary comment is perhaps necessary in view of the criticism by Lashley[1] and others of the anatomical and physiological assumptions involved in the conditioned reflex concept. I think we need not concern ourselves very much with these criticisms here. Pavlov has himself rather carefully refrained from too precise formulations as to the anatomical basis of the conditioned reflex and related phenomena that he describes, and his experimental data and the conclusions drawn from them seem to me to be in no way dependant upon any particular anatomical concept. It is with these experimental data rather than with any implied anatomical concepts that I propose to deal in this discussion.

Let us turn now to possible correlations between the two sets of findings.

The classical experiment upon which all of Pavlov's work in this field is based is, as is well known, the following: Pavlov has already demonstrated that if food be placed in a (hungry) dog's mouth, the stimulus of the food in the mouth excites a reflex secretion of saliva of a certain typical quality. This Pavlov calls an unconditioned reflex. If now just prior to putting food in the dog's mouth, the dog is exposed to some previously indifferent stimulus such as the sound of a metronome, this previously indifferent stimulus thereafter also acquires the power of exciting a secretion of saliva. This Pavlov calls a conditioned reflex.[2]

That the calling up of one reaction or mental process by another with which it has been previously associated, is essentially the

[1] Lashley: Brain Mechanisms and Intelligence, 1929.

[2] Except as otherwise noted, the following accounts of Pavlov's experimental results are abstracted from Pavlov: Conditioned Reflexes, 1927.

same phenomenon as Pavlov's conditioned reflex, is of course generally recognized and needs no further comment.

The relation between the inhibition of Pavlov and repression in the psychoanalytic sense is, however, not quite so clear. Pavlov distinguishes between two kinds of inhibition. If a dog for instance has been conditioned to respond with a secretion of saliva to the sound of an electric buzzer, but at the time of the sounding of the electric buzzer is exposed also to some other stimulus such as the entrance of a stranger into the room, then the sound of the electric buzzer will for the time being no longer possess the power of exciting a salivary secretion. This Pavlov calls *external inhibition*. If on the other hand, a dog that has been conditioned to the sound of an electric buzzer is repeatedly exposed to this stimulus without subsequently being given food, then the sound of the electric buzzer gradually loses its power of exciting salivary secretion. This is called by Pavlov the experimental extinction of a conditioned reflex. This is one of the forms of *internal inhibition*, the other forms of internal inhibition being in fact only modifications of this fundamental form.

To which of these forms of inhibition does repression the more closely correspond? The motive force in a great many cases of repression is fear (*e. g.*, castration complex). This would of course correspond to an external rather than to an internal inhibition, inasmuch as the inhibitory effect is called forth by a reflex other than the one that is being inhibited. In other cases, however, such as for example in the repression of the Edipus complex in women,[3] Freud conjectures that the repression results from the repeated failure of desires to be gratified. This would correspond more closely to the phenomena of internal inhibition.

But before proceeding to speak of repression in the psychoanalytic sense and inhibition in Pavlov's sense as equivalent terms, it will be well first to compare the two phenomena somewhat more closely. The essential feature of repression consists in the fact that although the repressed tendency is barred from obtaining direct motor expression, it nevertheless in indirect ways gives evidence that it still exists—as we say—in the unconscious. Are

[3] Freud, S.: The Passing of the Edipus Complex, 1924. Collected Papers, Vol. 2.

there similar evidences in the case of a conditioned reflex that has been inhibited in Pavlov's sense?

In the case of internal inhibition at least, there are many such evidences. A conditioned reflex that has been experimentally extinguished is not permanently destroyed. On the contrary a well established conditioned reflex will after a time spontaneously recover its full strength, and will do so more quickly if the animal is in the interim given food even without the conditioned stimulus. Moreover there is the interesting phenomenon that Pavlov calls disinhibition. We have noted for example that the coming of a stranger into the room will tend to inhibit the conditioned salivary response, let us say, to the sound of an electric buzzer; if, however, the stranger comes into the room after the conditioned reaction to the buzzer has been experimentally extinguished, then the effect is likely to be to "*disinhibit*" the salivary reflex, or in other words to release it from inhibition. Apparently the stranger's presence exerts an external inhibitory effect upon both the original reflex and the internal inhibitory process, and the more sensitive inhibitory process yields. It is clear, therefore, that internal inhibition just like repression involves a struggle between inhibiting and inhibited tendencies. Further evidence that this is the case is furnished by the following phenomena: First, it is possible to extinguish a reflex beyond zero; if one continues to sound the electric buzzer without following it by food even after the salivary reflex has been abolished, the later recovery of the reflex can be very much delayed. The struggle between inhibiting and inhibited tendencies is thus a quantitative phenomenon—just as in our psychoanalytic experience we find that there are varying depths of repression. In the second place, when one reflex is inhibited there is a definite tendency for the inhibition to radiate so that other conditioned reflexes also are temporarily inhibited—a phenomenon that corresponds to the repression of indifferent material so often noted immediately after allusions to deeply repressed trains of thought.

The psychoanalytic evidence for the fact that repressed tendencies have not been destroyed lies of course in the fact that such tendencies are continually finding outlets in symptoms and substitutive gratifications. It is therefore of interest to note the

analogy pointed out in detail by Ischlondsky[4] between symptom formation and the phenomena of generalization and of induction in Pavlov's experiments. Let us postpone for a moment the discussion of the generalization phenomena and explain briefly what is meant by induction. If a conditioned reflex, let us say to stimulation at one point on the skin, is inhibited, there is often observed immediately afterward an increase in the conditioned reflex effect obtained from other skin areas. It is easy to see that this phenomenon, which Pavlov calls *positive induction,* might well play an important part in such mechanisms for example as "displacement upward" of repressed genital excitations.

So far as we have been discussing internal inhibition. We have already noted, however, that the mechanism of many repressive processes would seem to resemble external rather than internal inhibition. Are there also facts relative to external inhibition that correspond to the phenomena of repression? Upon this point Pavlov's evidence is not so complete—but he does report one series of experiments that indicates definitely that such is probably the case. It is a well established fact in conditioned reflex experiments that painful stimuli as well as indifferent ones can be conditioned to excite the alimentary salivary response. In such cases the animal turns eagerly in the direction of the expected food and there is no evidence of defense reactions in spite of the painful nature of the exciting stimulus. In one series of experiments, an attempt was made to see how far this suppression of defense reactions could be carried, and not one but many conditioned reflexes were established to painful stimuli of increasing intensity applied to various points upon the skin. For a considerable time defense reactions continued to be inhibited and lively alimentary reactions continued to be obtained. Finally, however, a limit was reached and then the situation was completely reversed. The animal went into a prolonged state of restless excitement in which all types of stimuli were reacted to with violent defensive reactions and all alimentary reactions were inhibited. One could hardly seek a more effective demonstration of the fact that the defensive reactions had been "repressed" rather than destroyed.

[4] Ischlondsky, N. E.: Physiologische Grundlagen der Tiefenpsychologie, 1930. See especially chapter 2.

The evidence seems fairly complete therefore that repression in the psychoanalytic sense contains at least as one of its essential elements mechanisms such as Pavlov describes as external and internal inhibition. But does not repression also involve something more than this? Parlov's implied conception of external and internal inhibition as simple automatic reactions seems perhaps comparable to the avoidance of painfully charged topics in the talk of certain disintegrated schizophrenics, but one gains the impression in the study of the neuroses that repression is a more highly organized process than this.[5] We think of repression as an action of the ego[6] (in response it is true to a signal from the more automatic superego) and what is most important, we regard it as founded upon unconscious insight into the nature of the unconscious tendencies that are being rejected. Is there anything in the inhibition of conditioned reflexes comparable to this?

On the other hand, may we not perhaps ask ourselves (as Schilder[7] for example has done, and as some of the modern trends in psychology[8] suggest our doing) whether the conditioned responses of Pavlov are really so simple as they seem, and whether they also may not contain elements that correspond in at least a rudimentary way to the unconscious insight of our neurotic patients' superegos? I suggest, however, that we postpone the attempt to answer this question until we have discussed the phenomena that Pavlov calls differentiation and the light that they throw upon possible rudimentary parallels for unconscious and conscious insight.

Differentiation is a somewhat complex process of refinement of conditioned reflex responses whose most important component is a form of internal inhibition—differential inhibition. If an animal becomes positively conditioned to respond to a certain stimulus, say a tuning fork of a certain pitch, this conditioned response is at

[5] I owe the clear recognition of this distinction to a personal conversation with Alexander. See Alexander, F.: Psychoanalysis of the Total Personality, 1927. See especially chapters 3 and 4.

[6] Freud S.: Inhibition, Symptom and Anxiety, 1926.

[7] Schilder, P.: Conditioned Reflexes, Archives of Neurology and Psychiatry, 1929.

[8] Köhler, W.: The Mentality of Apes, 1921.

first (to use Pavlov's term) "generalized," that is, it can be obtained not only as a response to a tuning fork of that particular pitch but also as a response to a wide range of other stimuli as well. If now the animal is always fed after the tuning fork of the original pitch, but food is consistently withheld when the pitch is different, then the stimuli that are not followed by food are progressively "*differentially inhibited*," whereas the tuning fork of the original pitch is stabilized in its initial positively conditioned effect.

It is now of interest to note that differentiation in Pavlov's sense is an essential element in all sublimation—for sublimation, as Sachs[*] has pointed out, consists essentially in retaining the socially acceptable form of an originally undifferentiated tendency while rejecting unsocial forms of the same tendency.

The replacement of the Edipus wishes by the normal object choice at the time of puberty involves an essentially similar process. The boy's reactions toward women have been learned with reference to his mother. If his development has been approximately normal his sexual wishes toward her have been repressed, his tender impulses retained unrepressed. Thus at the beginning of the latency period, a differentiation in Pavlov's sense has already taken place. In addition to this differentiation, however, a generalization has also taken place and the repression of direct sexual aims that has been learned with reference to the mother has been extended as the pattern for the boy's attitude to other women as well. Throughout the latency period then—as we psychoanalysts put it—every woman is for the child a mother.

At the onset of puberty, however, the increased intensity of the sexual drive begins to disturb this generalized repression of direct sexual aims. We are fortunate to be able to cite at this point one of the few experimental observations that Pavlov has reported with reference to the effect upon the conditioned reflex phenomena of variations in the intensity of the animal's hunger. This observation is to the effect that when a differentiation has been established, an increase in the intensity of the animal's hunger tends to weaken the differential inhibition upon which the differentiation is based, and so to make of no effect the previously established differentia-

[*] Sachs, H.: Gemeinsame Tagträume, 1924. See also, Alexander, F.: Psychoanalysis of the Total Personality, p. 126.

tion. This is of course exactly parallel to the changes at the time of puberty that we are discussing. At this time the increased strength of the sexual drive threatens to break down the differentiation that during the latency period had repressed direct sexual aims and had allowed outlet only to the tender components of the sexual drive.

As a result of this weakening of the repressive barriers, the individual is now faced with the necessity of establishing a new differentiation—a differentiation that will discriminate this time as to the object of the sexual drive. If now development continues approximately normal, the sexual wishes toward the mother and toward other similarly inaccessible objects will indeed remain repressed, but sexual wishes toward other women will henceforth be granted outlet.

Thus it is evident that *the establishment of the normal object choice at the time of puberty is equivalent to a process of differentiation in Pavlov's sense.* It is further of interest to note the psychoanalytic observation that in situations in which the normal object love is thwarted (death of love object, disappointment in love, etc.) there is a tendency to regress again to the generalized infantile object choice and again to react toward all women after the pattern learned toward the mother. This is of course only another instance of the disappearance of differentiation as a result of an increase in the strength of the underlying drive.

Let us now return to the problem of insight. The more recent psychoanalytic literature has concerned itself a great deal with the functional division of the personality into ego, superego, and id. In these discussions we are accustomed to define the ego as the organ of adjustment to reality. What is there in Pavlov's experiments that corresponds to this adjustment to reality?

If we limit ourselves at first to external reality, the answer is easy. Pavlov has himself pointed out that the learning process is a process first of conditioning, then of differentiation of the generalized conditioned reflexes. By the acquisition of conditioned reflexes the hungry animal learns to seek for food in response to "signal stimuli" which have previously been associated with satisfied hunger; but not all stimuli that are at first treated as "signal stimuli" actually fulfil their implied promise to satisfy the animal's hunger. The second step in the learning process must therefore

consist in the (internal) inhibition of the responses to stimuli that are not habitually followed by satisfaction—in other words in a process of differentiation. It is thus evident that the process which in psychoanalysis we call *adjustment to external reality is based essentially upon the conditioning and differentiation processes of Pavlov.*

Furthermore we note that an animal that has acquired delicately differentiated conditioned responses actually acts as though it had a practical knowledge of external reality. In other words conditioning and differentiation do constitute a sort of rudimentary equivalent to the acquisition of insight into external reality—and we are again confronted with the question suggested by certain modern trends in psychology * as to whether insight in the true psychological sense may not be an essential feature of the conditioning and differentiation processes.

The external reality function of the ego is therefore based in its rudiments upon the processes of conditioning and of the formation of new differentiations that Pavlov describes. Does this fact help us to throw any light upon the nature of the ego or upon the conditions under which the ego can best function?

If we consider first the process of differentiation as Pavlov describes it, we are struck at once by the fact that differentiation takes place under conditions that psychoanalysts would call a conflict. Suppose we are attempting to differentiate a tone of pitch A as a positive stimulus over against a tone of pitch G as an inhibitory stimulus. Due to the initial generalization of both excitatory and inhibitory responses both A and G at first call forth both excitatory and inhibitory reactions—and there will thus exist at the outset a conflict between the excitatory and the inhibitory reactions. The process of differentiation will then consist in the gradual narrowing of the initial generalizations and in the concentration of the positive responses to the stimuli about A and of the negative responses to the stimuli about G. We see, therefore, that differentiation is a process of solution of a conflict.

There is a similar fact with reference to conditioning. An animal tends to form alimentary conditioned reflexes only when it is hungry. In other words an important condition for the conditioning

* See references given above (page 1171).

process is an unsatisfied drive—a condition that we may characterize as a conflict with external reality.

We may postulate, therefore, that one of the necessary conditions for the external reality function of the ego is the existence of either an external or an internal conflict.

Certain other observations made by Pavlov allow us to postulate another condition for the external reality function of the ego. Pavlov states that one of the necessary conditions for the formation of a conditioned reflex is that the animal must be alert. If the animal is not in good health, therefore, or if there are powerful extraneous stimuli, the formation of a conditioned reflex is interfered with or prevented. A conditioned reflex is typically a reaction acquired to a *previously indifferent* stimulus. In order for conditioning to take place, therefore, it is important that there shall not be prepotent reflexes strong enough to prevent the animal's reacting to essentially neutral stimuli. In psychoanalytic terminology we might say, therefore, that a necessary condition for this most elementary of the adjustments of the ego to external reality is the existence of free psychic energy [10] that can be turned to *previously indifferent* stimuli.

That a similar condition exists for the formation of differentiations is strikingly illustrated by Pavlov's experiments upon the production of functional pathological states. These pathological states are of two sorts. One consists in an excessive predominance of excitation, so that the animal reacts to all stimuli (even previously inhibitory stimuli) with excitation. In the other type of pathological state, on the other hand, the animal reacts to all stimuli with inhibition. Either of these two types of pathological disturbance not only makes impossible the formation of new differentiations or conditioned reflexes, but also destroys for a prolonged period existing differentiations. Interesting for our present argument is the fact that the procedure for producing these pathological states consists regularly in throwing the animal into what psychoanalysts would call an acute conflict. The procedures commonly used are either (1) to attempt to force the animal to form differentia-

[10] The concept "psychic energy" has been found difficult to reconcile with the psysiological data at present available. See Lashley: Physiological Analysis of the Libido, Psychological Review, 1924. See also below (page 1191) for further brief discussion of this point.

tions that in number or fineness of discrimination exceed its capacity; or (2) to exceed the animal's capacity to react to painful stimuli as conditioned alimentary stimuli. It requires no further comment to make it evident that in both of these situations opposing reactions are called forth in such intensity that further conditioning and differentiation become impossible. What apparently happens is that each of the opposing reactions becomes generalized widely, and the stronger of the two or some resultant reaction becomes a stereotyped generalized reaction to all stimuli. Paraphrasing now into psychoanalytic terminology in accordance with our argument we may say that the conflict has now become too intense and generalized to leave any energy free for ego functioning.

The external reality function of the ego would thus appear to be *dependent upon two conditions:*

(1) *There must be conflict, either external or internal;* in other words, the animal must be in a state of deprivation; and

(2) *The conflict must not be too intense; there must be sufficient energy left free to allow the animal to retain its alertness to previously indifferent stimuli.*

This analysis of the external reality function of the ego, enables us, moreover, to carry one step further our comparison of repression with external and internal inhibition. In psychoanalysis we are accustomed to distinguish between the repression and the conscious rejection of a wish or impulse. The conscious rejection of an impulse is from beginning to end an act of the ego in accordance with its function of adaptation to external reality. This adaptation to external reality we have seen is based upon the formation of differentiations in which *previously indifferent* stimuli (*i. e.,* the real external situation) become the arbiters as to whether or not the particular tendency shall be inhibited. Repression on the other hand is characterized as a "flight" of the ego resulting in a withdrawal of the repressed tendency away from the reality testing function of the ego. Repression therefore corresponds to those functional pathological states in Pavlov's experiments in which the "conflict" is too intense to allow essentially neutral stimuli (*i. e.,* real external situation) to become arbiters in the process of differentiation. We have pointed out above that the phenomena of internal inhibition give evidence of a struggle between inhibition

and excitation (*i. e.*, between inhibiting and inhibited tendencies). This is particularly true during the phases of initial generalization of both excitatory and inhibitory reflexes. If I understand Pavlov rightly, a completed differentiation involves a concentration of both excitatory and inhibitory reflexes each about its specific differential stimuli, in other words in a "solution" of the initial "conflict." It is the inhibitions incident to such differentiations ("conflict solutions") that correspond to the phenomena of conscious rejection; whereas the phenomena of repression correspond closely to the inhibitions resulting from the chronically "unsolved conflicts."

Before we proceed further with analogies between psychoanalytic findings and those of Pavlov, however, there are important objections to be considered. Are the phenomena that we are comparing really comparable?

Pavlov is discussing rather elementary automatic reactions, usually to external stimuli. Psychoanalysis deals with wishes and impulses arising from the great diffuse drives, such as hunger and sex, within the organism. Such wishes often manifest themselves in dreams or phantasies rather than in any overt action. The activity to which a wish gives rise is moreover goal-directed, tending to persist until the wish is fulfilled, and if reducible at all to reflex terms involving not one but many reflexes. Any serious attempt to correlate Pavlov's work with psychoanalytic findings must therefore undertake at least to sketch a sort of "structural formula" of a wish and of its fulfilment mechanisms in the terms of Pavlov's data.

Before attempting this, however, it is well to realize that the phenomena with which Pavlov is dealing are not so simple as they might seem. The alimentary salivary reflexes investigated by Pavlov are of course only one small element in the food-seeking reactions of the animal studied. Pavlov notes the accompanying alimentary motor reaction, but if the animal were released from the stand we know that it would be likely to run to its food, and even if no adequate conditioned stimuli were at hand, we know that a really hungry dog would proceed to seek for food until it either was satisfied or was diverted by fatigue or otherwise from the attempt. It becomes evident immediately, therefore, that the alimentary salivary reflexes that Pavlov studies are only isolated elements in a more complex goal-directed activity.

It seems probable, moreover, that we are also under-estimating the complexity of the conditioned responses that Pavlov describes. We have already pointed out that Pavlov's formulation of the conditioning process seems to take inadequate account of the animal's hunger. Pavlov apparently takes it for granted as a necessary condition of his experiments that his dogs are sufficiently hungry, and speaks as though he thought of the alimentary salivary secretion as conditioned to the formerly indifferent external stimulus alone. We know however, that a hungry man will dream of food, and it is thus evident that the state of hunger itself must play an important part at least in sensitizing the receptors for the conditioned stimulus. We must therefore probably think of the conditioning process in Pavlov's experiments as a double one, composed of two more elementary conditioning processes: (1) the state of hunger becomes conditioned to sensitize the receptors for the conditioned stimulus to respond more readily; and (2) the conditioned stimulus becomes conditioned to excite in its turn the salivary secretory reaction.

Let us examine this suggestion in the light of the theory of dreams. Freud [11] has formulated the hypothesis that the wish-fulfilling hallucination or dream arises in the simplest cases in reaction to an unsatisfied drive by hallucinatory reproduction of a previous experience in which this drive was satisfied. What Freud here postulates is obviously analogous to a conditioned reflex reaction. A hungry man dreams of food. The man has been hungry before and his hunger has been followed by the sight of food and then by satisfaction of his hunger. Pavlov has in fact demonstrated that internal physiological states may acquire conditioned reflex properties, and it therefore seems not unreasonable to suppose that the state of hunger itself might become conditioned to elicit some sort of a reproduction of a previous satisfying experience.

Dreams and hallucinations, however, are subjective phenomena. Conditioned reflexes are objective motor or secretory reactions. Are there any objective evidences that dreams and hallucinations elicit reactions similar to those that would have been elicited by the corresponding real stimuli?

[11] Freud, S.: Interpretation of Dreams, 1913, Chapter 7, section (c), The Wishfulfillment.

This question must necessarily recall to our minds one of the most fundamental peculiarities of unconscious mental processes—the fact that, in the system Ucs, wish and fact, psychic and external reality, are treated as equivalent. A dream does satisfy the dream wish for a time at least, just as though it corresponded to reality; only in this way, in fact can we explain the dream's success in prolonging sleep. Moreover, we know that unconscious phantasies call forth reactions of fear, guilt,* etc., such as would correspond, at least qualitatively, to their actual realization. Thirst may for example call up a dream of a cooling drink that may for a time give considerable relief; and a passive sexual wish as in Freud's well-known case,[12] may give rise to a vivid dream calling forth

*The unconscious insight of the superego into the nature of repressed tendencies is of course only another name for the tendency of the superego to react to unconscious phantasies with guilt, fear, self-punishment, etc., as though the phantasies corresponded to external reality.

There is one point, however, in which our psychoanalytic knowledge of the process of repression goes beyond any analogies that physiological studies are as yet able to give us. Repression is sometimes characterized (Freud—Interpretation of Dreams, Chapter 7, section (e), Primary and Secondary Processes—Repression) as a flight of the ego from painful mental contents. As such we have seen it offers little to distinguish it from the external and internal inhibition in Pavlov's experiments; and in the case of simple phobias, for example, the repressive process seems hardly more complicated than this.

In many instances, however, in addition to the flight of the ego from the (originally external) danger associated with the repressed impulse, there is also an active rejection of the impulse by the superego (This is of course seen most plainly in aggressive impulses to self-punishment—such as suicide). This active rejecting rôle as Freud has pointed out in his account of the origin of the superego (The Ego and The Id) is based upon an identification with the forbidding parent. The activity involved in this identification with the rôle of the forbidding parent is moreover beautifully illustrated in what might be called a sort of intermediate stage in the development of the superego—in which children tend to impose first upon other children the prohibitions and punishments that they have received from their parents.

This process of identification with the behavior of others which is so important for the understanding of our psychoanalytic findings, has as yet, however, so far as I am aware, received no thorough-going experimental physiological analysis—easy as it might be to speculate upon possible mechanisms involved.

[12] Freud, S.: From the History of an Infantile Neurosis, 1918. Collected Papers, Vol. III.

fear such as the real " wolf " might have caused. It appears therefore that we must take quite seriously the idea that hunger may become conditioned to elicit the reactions corresponding to stimuli associated with its satisfaction.

The equivalence of psychic and external reality, however, holds good only for unconscious mental processes, or more technically speaking, only for the system Ucs. Conscious mental processes, or the system Cs are in fact specifically characterized by the ability to test reality, to distinguish between reality and wish-fulfilling phantasy.

This corresponds, however, to another point that must necessarily follow according to our hypothesis. Dreams and unconscious phantasies do not as a rule lead to real satisfaction. If hunger has been conditioned to elicit a hallucination and the reactions appropriate to the corresponding external stimulus, we must therefore assume that this conditioned reflex will soon be differentially inhibited in contrast to motor reactions that are more successful in bringing real satisfaction. The wide awake child no longer dreams of sweets but looks for them in the pantry. The above, however, is merely a physiological description of what in psychoanalysis we call the function of testing reality.

There can now be only one possible explanation for the inability of the system Ucs to distinguish between wish and reality. We have found that *the reality testing function is based upon the establishment of a differentiation to inhibit conditioned reactions that treat wish-fulfilling phantasies as equivalent to reality. The loss of the reality testing function in the system Ucs must therefore be due to conditions that destroy this differentiation.**

What is here suggested is of course a physiological explanation for the distinction between system Cs and Ucs. Our physiological hypothesis confirms Freud's conclusion that the distinction is a dynamic rather than a topographical one. The system Ucs, however has other peculiarities than the fact that it cannot distinguish between wish and external reality. It functions according to the primary psychic process which differs in many ways from the secondary psychic process characteristic of the system Cs. The primary process[13] is characterized by the ready displacement of

* See note below (page 1182).

[13] See Freud: Interpretation of Dreams, chapter 7, section (e), also The Unconscious. Collected Papers, Vol. IV, 1915.

psychic energy from one mental element to another, by the ready condensation of mutually contradictory elements, by timelessness and by the lack of distinction between psychic and external reality. Are any of Pavlov's data comparable to the peculiarities of the primary process?

We have already referred to the functional pathological states or "experimental neuroses" in Pavlov's dogs, and to the fact that these may be either excitatory or inhibitory in character. Similar to the inhibitory types there are also what Pavlov calls "partial sleep states"—induced by limiting the generalization of internal inhibition upon which normal sleep is based. The chief difference between these partial sleep states and the inhibitory type of experimental neuroses lies in the shorter duration of the former.

In either of these types of inhibitory state it occurs frequently that a dog will react with a considerable salivary secretion to a weak stimulus that ordinarily would have elicited only a very slight secretion, whereas a strong stimulus that ordinarily would have elicited a considerable secretion is now reacted to with little or none. This Pavlov calls "the paradoxical phenomenon." Sometimes this peculiarity is even more pronounced in that a stimulus that ordinarily would have inhibited secretion now excites secretion, whereas a previously excitatory stimulus becomes inhibitory—"the ultra-paradoxical phenomenon" of Pavlov. In other cases the effect is less pronounced in that all stimuli—weak and strong alike—elicit approximately equal reactions—"The equalization phenomenon" of Pavlov.

It is of course very difficult to compare phenomena of this sort, based wholly upon objective and external observation, with the much more complex phenomena that characterize the primary process of mental functioning of the system Ucs. Nevertheless these paradoxical and equalization phenomena of Pavlov's dogs are at least suggestive of two of the specific peculiarities of unconscious mental processes: (1) the representation of an unconscious psychic element by its opposite, and (2) the displacement of emphasis from psychic elements heavily laden with affect upon relatively indifferent elements. The probability that these two groups of phenomena are essentially equivalent is enhanced further by the similarity of the conditions under which they occur: in psycho-

analytic experience, in neuroses (including psychoses) and dreams; in Pavlov's experiments, in "experimental neuroses" and "partial sleep states." *

If these two groups of phenomena are equivalent, an interesting light is thrown upon the phenomena of repression. We have called attention to the fact that the internal inhibition of a conditioned reflex involves a struggle between inhibiting and inhibited tendencies. This is of course especially the case before a particular stimulus has become established as an inhibitory stimulus. It is now of interest to cite the report of Ischlondsky [14] that the above described paradoxical and equalization phenomena occur not only in experimental neuroses and partial sleep states but also as transient phenomena during the process of establishment of new differentiations. The analogy with psychoanalytic findings is again

* This discussion must obviously also raise the question as to just what are the conditions that cause the differentiation between reality and phantasy to break down in neuroses and in dreams.

In the case of neuroses, the break-down of this differentiation corresponds to the above discussed general tendency of conflict to break down existing differentiations. Clinical and physiological evidence agree, moreover, also in the fact that neuroses may be either predominately excitatory (manic or catatonic excitements) or predominantly inhibitory (depressions, catatonic stupors, etc.) depending upon the excitatory or inhibitory character of the undifferentiated reactions called forth by the (conscious or unconscious) phantasy presentations.

In the case of dreams, it is not immediately clear just why the generalization of internal inhibition upon which sleep is based should favor the undifferentiated tendency to react to phantasy presentations as though they were real. The clue to the explanation lies probably in the fact that these undifferentiated reactions to the phantasy presentations are themselves inhibitory in character. The wish-fulfilling dream is by virtue of this very fact sleep-conserving. In other words, satisfying a wish exerts a quieting or inhibitory effect and this "inhibition of satisfaction" (as we may call it) apparently fuses readily with the generalized internal inhibitions upon which sleep is based.

This explanation, however, raises the further question (which will be discussed briefly below—footnote, page 1187) as to the exact nature of the relations between this "inhibition of satisfaction" and the inhibitory reflexes in Pavlov's experiments. As indicated below, this point requires further experimental investigation. It seems probable, moreover, that a clinical study, especially of dream material, might throw considerable light upon it.

[14] Ischlondsky, N. E.: Der Bedingte Reflex, 1930. See page 279.

suggestive—for we have already pointed out that the phenomena of repression (in contradistinction to those of conscious renunciation of a rejected tendency) correspond to those of an incomplete differentiation or "unsolved conflict" and repression also relegates a mental process to the system Ucs.

We have as yet, however, only half sketched the "structural formula" of a wish. We have traced the origin of the wish-fulfilment phantasy as a conditioned reaction to the hunger state, and we have observed how the tendency to react to this phantasy as though it were real has been differentially inhibited. When this phantasy or hallucination ceases to be reacted to as though it were real, what becomes of it? Psychoanalytic experience tells us that it may now become a conscious day-dream, a wish or a purpose. But as a wish or a purpose it acquires new properties. It becomes now a goal for action. The child's dream of sweets, for example, upon being inhibited as a wish-fulfilling hallucination, becomes now a goal and incentive directing the child to the pantry. Can we understand this in terms of Pavlov's data?

Certain of Pavlov's experiments help us to make a first step toward the answer to this question. If an animal is fed regularly—say three minutes after the sounding of a gong, there results what Pavlov calls a delayed conditioned reflex. During the first two and a half minutes or more there will be no salivary secretion; but during the last few seconds an abundant secretion appears. It is possible to demonstrate, however, that during the period during which there is no secretion, the secretary reaction is nevertheless latent but internally inhibited. This type of internal inhibition is called the *inhibition of delay*.

It is possible, moreover, so to condition an animal that a stimulus such as for example an electric gong will be inhibitory when exhibited alone, but will have a marked positive secretory effect in combination with, let us say, the sound of bubbling water. In other words, the compound stimulus—gong plus bubbling water—acquires a positive secretory effect that may be quite independant of any positive effect of either gong or bubbling water when exhibited alone.

These experimental findings of Pavlov furnish close analogies to the mode of action of a wish, purpose or other goal presentation. As an urge inciting to appropriate activity, the goal presentation

acts essentially as a constantly present element in a series of compound stimulus combinations. To choose a simple example[25] a student wishing to study will react otherwise to an electric light switch in a dark room than will a student wishing to sleep. The wish to study plus the electric light switch, thus form a compound stimulus inducing the student to turn on the light; whereas neither the electric light switch nor the wish to study, alone, would have given rise to this action.

A second function of the goal presentation is an anticipatory or preparatory one. The student looking for the electric light switch will be more likely to find it and react to it than if he were not looking for it. The wish to find the switch implies already an anticipatory sensitization to it. Sometimes also one becomes impatient or overconfident and reacts as though a desired goal were already achieved—as in the case of the well-known tendency to overconfident exultation and relaxation of effort upon the eve of victory; facts which show clearly that the tendency to react to the goal as already realized, is, though inhibited, nevertheless latent just as in the case of Pavlov's delayed conditioned reflexes.

The mechanisms so far discussed, however, explain the guiding function of the goal presentation only as a static function after fixed reaction patterns have been formed. In the example given above, for instance, the student presumably already knows that the pressing of the electric light button will turn on the light. Pressing the button, therefore, is already a relatively fixed pattern reaction to the combination-wish for light plus electric light button.

A more difficult and fundamental problem concerns the influence of the goal upon the learning process. Let us suppose for example

[25] This point has also been stressed by Woodward (Woodward R. S.: Dynamic Psychology, 1924). Woodward, however, wrote before the publication in English of Pavlov's Conditional Reflexes and did not bring his discussion so closely into relation with Pavlov's experimental data.

The tendency of the "drive stimulus" to call forth premature anticipatory reactions as though the goal were already present, has been stressed by Hull (Hull, C. L.: Goal Attraction and Directing Ideas Conceived as Habit Phenomena, Psychological Review, 1931) and has been made by him the basis of a theoretical reconstruction of the mechanisms of goal-directed behavior, which is in some respects similar to the one here presented. Hull, however, looks upon the goal presentation as a part of the anticipatory reaction that has *escaped inhibition* and does not recognize therefore, the *contrast between the goal presentation as wished for and the achievement of the goal.*

that the electric light switch is of a kind different from any with which the student is already familiar and that the student is uncertain which of several buttons or levers he should manipulate. He tries several and finally finds the particular switch that will turn on the electric light. By this process, however, he has presumably learned something. The next time he wishes light in this room, he will probably proceed immediately to the correct switch and avoid all manipulations that failed to bring the desired light.

I choose this very simple and trite example in order to bring into relief both how it resembles and how it differs from the processes of conditioning and of differentiation described by Pavlov. The similarities will impress us first. The light corresponds to the unconditioned stimulus (*e. g.*, food in the mouth) in Pavlov's experiments; the desire for light, to the animal's hunger. In Pavlov's experiments, to be sure, the response (salivary secretion) is a relatively fixed one and the effect of the conditioning process is to give certain *stimuli* a positive effect and others an inhibitory effect upon salivary secretion; whereas in our example, the differentiation takes place between different *motor responses.* Nevertheless, just as the unconditioned stimulus (food in the mouth) in Pavlov's experiments converts the stimulus just preceding it into a positively conditioned stimulus, so does the light in our example, "condition" the student to turn next time to the correct switch: and just as in Pavlov's experiments other *stimuli* acquire inhibitory properties because they are not followed by the unconditioned stimulus, so in our example does the student learn to dispense with *manipulations* that failed to bring the light. Upon the basis of these analogies, therefore, Ischlondsky [18] quite unhesitatingly speaks of motor reactions as well as stimuli as capable of becoming positively conditioned.

There is one important difference, however, between such "conditioned" motor responses and the conditioned reflex phenomena of Pavlov as worked out in experiments upon the salivary reflexes. According to Pavlov's original formulation a conditioned stimulus acquires the reflex properties of the unconditioned stimulus upon

[18] Ischlondsky, N. E.: Der Bedingte Reflex, p. 62-63. Physiologische Grundlagen der Tiefenpsychologie, p. 64.

which it is based. This seems quite correct so long as one speaks only of the salivary secretory reflexes, inasmuch as the reflex response acquired by the conditioned stimulus in this instance is indeed "the secretion of saliva of the same quality and character" as that elicited by the unconditioned stimulus. The same could be said of reflexes conditioned upon avoidance and defense reflexes as unconditioned stimuli—the animal in these cases, *e. g.*, withdrawing its paw in response to conditioned stimuli just as it does in response to the unconditioned stimulus (electric shock, etc.)

In our example, however, something quite different occurs. The "conditioned" motor response in this instance is to push the button to *turn on* the light.* The reaction *to the light* will be quite different—perhaps sitting down and starting to study. The effect of the light as a reenforcing stimulus is to "condition" positively reactions that *result in* or *are followed by* the light [17] rather than to condition other stimuli to elicit the same response as the light itself.

I suspect, in fact, that if Pavlov had turned his attention more to the alimentary motor reflexes, he would himself have been compelled to modify or supplement his original formulation. If the conditioned stimulus in this case is the taking of food into the mouth then the animal's "turning his head toward the expected

* For the sake of simplicity I have purposely relegated to this footnote discussion of another difference between our example and the conditioned salivary reflexes of Pavlov. The light in our example is not an *unconditioned* stimulus. The motor response to the electric light switch must therefore be compared with the *secondary* conditioned reflexes of Pavlov, *i. e.*, conditioned reflexes secondarily conditioned upon the basis of a conditioned rather than of an unconditioned stimulus. A further problem is created by the fact that such secondary conditioned reflexes are established with much difficulty in Pavlov's experiments—in marked contrast to the stability of much goal directed behavior in human beings. Further investigation would seem to be needed to determine whether this is due to differences between dogs and human beings or to differences between salivary and motor reflexes or to other unknown factors.

[17] In the American literature, this has been called by Thorndike the "law of effect." Thorndike, E. L.: Educational Psychology, 1921, Vol. I, Chapter 12. The contrast between this "law of effect" and the conditioning principle of Pavlov is particularly clearly brought out by K. A. Williams: The Conditioned Reflex and the Sign Function in Learning, Psychological Review, 1929. The "sign function" corresponds roughly to the rôle of induction in the present discussion.

food and smacking his lips" cannot be a conditioned reproduction of the reflex elicited by the unconditioned stimulus—for the unconditioned reaction to taking food into the mouth is mastication and swallowing. The answer of the physiologist to this criticism might possibly be that in the case of the alimentary motor reflex the unconditioned stimulus is not the taste but the sight or smell of the food and that the turning of the head toward the food is indeed an unconditioned reaction to this stimulus. I suspect, however, that the effect of the sight or smell of the food in this case merely obscures the relations we are here discussing—for in the case of the maze experiments of American experimental psychologists—in which the actual reaching of the food is plainly the effective reenforcing unconditioned stimulus—the choice of the correct path through the maze is obviously a very different reaction from that of the animal after it has found the food. It would seem therefore that in these cases we must substitute for Pavlov's original formulation, the more popular formula of common sense psychology that it is those motor reactions that lead to the satisfaction of a drive or a wish that tend to become positively conditioned whereas those reactions that are not followed by the desired satisfaction tend to become inhibited.[15]

But satisfaction is a subjective psychological concept. To what objective physiological phenomena does it correspond?

Let us consider somewhat more closely the function of the unconditioned stimulus in Pavlov's experiments. The function of the stimuli presented by food in the mouth would appear to be twofold. On the one hand are the phenomena upon which Pavlov's experiments lay chief emphasis; introduction of food into the mouth *initiates* the processes of mastication and salivary digestion for which of course salivary secretion is a necessary preliminary. But the introduction of food into the mouth exerts another influence that is equally important; it puts an end* for the time to

[15] This formula corresponds roughly to the pleasure principle of Freud or to modified forms of the pleasure principle such as the reality principle. Pavlov's original formulation correlates roughly with the repetition compulsion of Freud. See Freud: Beyond the Pleasure Principle, 1920.

* Pavlov has apparently only just begun to investigate this inhibitory effect of the unconditioned stimulus, and in fact appears to be investigating it only indirectly—still by means of experiments upon the more easily measured salivary reflexes. One series of experiments is, however, suggestive—in that

the food-seeking motor activities of the animal. It is obviously this inhibitory effect of the unconditioned stimulus upon the food-seeking activities of the animal that is the physiological correlate to the satisfying psychological character of this stimulus; and the well-known soporific effects of satiety (gastronomical or sexual) point to still more widely generalized inhibitory effects of the internal stimuli corresponding to satiety.

It will be of interest to consider these relations in some detail with reference to the alimentary motor reactions. The *initiatory* unconditioned stimulus for these motor reactions is probably the combination—hunger plus the sight or smell of food. This initiatory combination obviously has an *excitatory* effect. In contradistinction to this excitatory initiatory combination, the introduction of food into the mouth as already pointed out serves as a *terminal* stimulus whose function is an *inhibitory* one, *to put an end to* the alimentary motor reactions in question.

We now note that it is the *inhibitory terminal* stimulus that is capable of exercising a positive conditioning or reenforcing effect upon successful motor reactions; but it is again the *initiatory* combination whose power to elicit the successful motor reaction is reenforced. The " satisfying " terminal stimulus, however, stands in a relation of direct contrast to one essential element in the initiatory combination. Hunger implies the lack of food; the desire for light, in the example of the student, is, one might say, the negative or opposite of the reenforcing stimulus, the light. The reenforcing influence of the " satisfying " stimulus, light or food, is thus exerted in favor of the power of its *polar opposite* to elicit a motor reaction *upon which the light or food itself exerts an inhibitory effect.*

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it indicates that a previously positively conditioned stimulus tends to lose its positively conditioned properties and even to become inhibitory for both salivary and motor alimentary reflexes, if exhibited regularly *immediately after* the unconditioned stimulus. The inhibitory effect of the unconditioned stimulus upon stimuli immediately following, Pavlov is inclined to regard as an external inhibition—a suggestion that might imply that the inhibitory effect of food in the mouth upon the food-seeking motor activities of the animal is an external inhibitory effect due to the prepotent masticatory and secretory reflexes initiated by this unconditioned stimulus.

Certain other well-known facts, however, such as the soporific effects of satiety (gastronomical or sexual) would seem to indicate that the inhibitory effect of the satisfaction of the fundamental drives has a much more fundamental biological basis.

Such detailed exposition of facts that are matters of everyday knowledge * probably seems exceedingly pedantic. Such an exposition is necessary, however, in order to bring out clearly an analogy that seems to me to be of considerable significance. We are all familiar with the phenomena of mutual spinal induction demonstrated by Sherrington,[19] by virtue of which the activity of one muscle group is accompanied by the simultaneous reflex inhibition of antagonistic groups. In the light of the above exposition, I am inclined to believe that the fundamental biological tendency of living organisms to strive toward the satisfaction of drives is based upon mechanisms of the same general sort as the spinal induction mechanisms of Sherrington. Just as the activity of one muscle group and the inhibition of its antagonist form together an organic functional unit reaction; so also, even more fundamentally, do the inhibitory influence of a "satisfying" stimulus group and the excitatory influence of the need or hunger for that group of stimuli constitute a functional unity—such that the inhibitory influence of the "satisfying" stimulus necessarily involves a reenforcement of the excitatory influence of the "need" for it—with reference to any particular motor reaction.

In the analysis of dreams one finds frequently that the dream wish is a direct denial of the unpleasant experience that was the immediate exciting cause of the dream[20]—an observation that suggests definitely that the wish fulfilling function of the dream is based upon a mechanism of induction, the wish fulfilling hallucination being in direct contrast to the "need" that calls it forth.

We have already seen that the wish-fulfilling phantasy presentation is capable of functioning in either of two different ways. When the satisfying dream of food fails to be followed by real food, its function as a satisfying substitute for food is inhibited and it becomes then a goal spurring the individual to seek for food. We now notice that these two functions are not only alternate

* The writer of course realizes that such "everyday knowledge" is only rough and approximate. More precise working out of the points here discussed must await further experimentation. See footnote, page 1187.

[19] Sherrington, C. S.: The Integrative action of the Nervous System, 1923.

[20] To cite only one example, see Freud's analysis of his own dream that he is riding horseback—dreamed in reaction to a boil upon the buttock. Freud, S.: Interpretation of Dreams, Chapter 5 (c), Somatic Sources of Dreams, pp. 193-194.

functions; they also stand in polar contrast to each other. The wish for food sets in action just those motor reactions that the dream of food inhibits. The activation of these motor reactions would therefore quite plainly seem to be an effect by induction of the inhibition of the "satisfying" function of the dream.

We have already cited the fact that, for example, inhibition of a conditioned reflex to stimulation at one point on the skin, often gives rise by induction to an increase in the conditioned reflex effect obtained from another skin area—and especially a very distant or a very near skin area. We have also noted that the wish for light in the case of our student, functions (1) analogously to a delayed reflex *to the light* during the inhibitory period of latency and (2) as one element in a series of compound stimuli, sensitizing the student to react to the otherwise indifferent electric switch. It will be noted that the analogy to the above mentioned induction phenomena in Pavlov's experiments is very close; and that the sensitization of the student to the electric switch would seem quite plainly to be an effect by induction of the inhibition of the conditioned reaction to the light.

Upon the basis of the above discussion, therefore, we feel justified in advancing the hypothesis that *the activating influence of the wish upon motor activities directed toward its realization is a direct effect by induction of the inhibition of its reality value.*[21]

There is a direct evidence also in Pavlov's experimental findings that hunger, like its derivative the wish, is a source of radiation of excitation. Pavlov reports that increase in the animal's hunger tends to break down internal inhibitions of all kinds. It is evident therefore that the hunger itself must give rise to a rather widely distributed radiation of excitation. Is it possible, perhaps, that the diffuse alertness that Pavlov postulates as a necessary precondition if new conditioned reflexes are to be formed, is also a manifestation of just such a widely distributed radiation of excitation from hunger and other drives?

One of the most fundamental physiological objections[22] to the psychoanalytic formulations has been directed against the psycho-

[21] This inference is suggested by a number of examples cited by Ischlondsky (Physiologische Grundlagen der Tiefenpsychologie, pp. 51-57), but is not definitely stated by him.

[22] Lashley: Physiological Analysis of the Libido, Psychological Review, 1924.

analytic assumption of a free psychosexual energy that can be transferred from one wish or activity to another. The phenomena of substitutive gratification, regression and sublimation seem to make such an assumption inevitable, but it has nevertheless been very difficult to reconcile this assumption with any known physiological data. The evidence just adduced for a widely distributed radiation of excitation corresponding to the hunger state, points the way to a fruitful investigation of this problem. Pavlov has already worked out very comprehensive experiments upon the progress of irradiation of excitation and inhibition. If experiments could be devised to work out the relations between the *extent and intensity of such irradiations* on the one hand, and factors such as *hunger* and *fatigue* on the other, we suspect that such investigations might throw considerable light upon the physiological nature of the " psycho-sexual " energy that psychoanalysis must postulate. From our psychoanalytic evidence, we should be inclined to suspect, for instance, that the effect of external stimulation and of previous conditioning would consist in large part in the *concentration*[23] *in the neighborhood of particular stimuli,* of some of the diffuse radiation arising from the hunger state. If this expectation should prove to be correct, then the " functional mosaics " of Pavlov would represent *patterns of distribution* of excitation and inhibition, the total amount of which would depend upon the state of satisfaction of the various drives, the extent and degree of fatigue and perhaps other similar factors.

For the sake of clarity it will be well at this point to bring together and summarize the several suggestions here made with a view to a theoretical reconstruction of the mode of functioning of a drive and especially of those drives such as hunger or sex whose activity involves the seeking of an object.

A drive may be called into activity in one of two ways: either (1) (provided the animal is not completely sated) by an external stimulus such as the sight of food; or (2) by the development of

[23] See Thurstone, L. L.: The nature of Intelligence, 1926. Thurstone, in agreement (at least to this extent) with the assumptions underlying psychoanalysis, points out that behavior arises from *diffuse* inner tendencies (such as hunger) which then become more and more *concentrated* in reaction to external stimuli.

a need as a result of the more or less prolonged absence of gratification. Once called into activity, the drive now tends to give rise to a diffuse radiation * of excitation which makes the animal alert or sensitive to a wide range of external stimuli. The diffuse distribution of this excitation is, however, combated by tendencies toward concentration which are in general of two sorts. (1) There is the tendency toward the concentration of the energy of the drive upon dreams or wishes derived from previous experiences of satisfaction, or in more physiological terms upon the receptors corresponding to stimuli that have been previously positively conditioned. (2) On the other hand, we must postulate an equally important tendency to concentration of excitation about the external stimuli actually presented at the moment, a tendency to concentration which is of course increased if the external stimulus presented is a positively conditioned one.

We have pointed out that a wish and an appropriate direct stimulus (*e. g.*, the student's wish for light plus the electric switch) function as two elements in a compound stimulus. I should like to supplement this analogy with another one which I suspect is not inconsistent with it and has also the advantage of accounting for a greater number of facts. There is much to suggest that a wish and the external stimuli presented at the moment, act by virtue of some sort of difference of potential that tends to activate pathways between them. Thus my hunger may activate wishes for a particular food, for a restaurant where that food is particularly well served, and for ways of getting to that restaurant. But this forms only one component in the determination of my behavior; for my choice of route will in any case be determined in an equal degree by my location at the moment and even my choice of restaurant and of food wished for must be influenced by their availability.

You will recall that Freud [24] has shown that a dream must fulfil two conditions. (1) It must represent the fulfillment of a wish dating from childhood, and (2) it must contain references to material of the day preceding. Thus in dreams also, we seem to see a suggestion that there is at work a sort of potential difference between a wish and recent direct stimulation; and the tendency to

* Such diffuse "radiation" might conceivably take place either by way of nervous pathways or by means of internal secretions through the blood stream.

[24] Freud, S.: Interpretation of Dreams.

hallucinatory reproduction of previous satisfying experiences as though they corresponded to external reality, must probably be regarded as another manifestation of the same sort of dynamic polarity between wish and direct stimulation. (That the wish must be an infantile one, we would explain by the fact that infantile wishes form the undifferentiated basis for all later ones.)

This suggestion that wish and external reality function as poles activating pathways between them can readily be brought into relation with a recent suggestion of Lashley's. Upon the basis of very extensive experimental investigations, Child [25] has demonstrated that "physiological gradients from points of higher to points of lower excitation" establish themselves as the first step in embryological development, determine the axes of growth, control the details of development and, in the nervous system, predetermine the pathways of conduction before the anatomical connections have been established. Lashley [26] has tentatively suggested that such physiological gradients play an important part in the function of the cerebral cortex. I am tempted to follow up this suggestion with the further one that the psychological tension between wish and external reality corresponds to the dominant physiological gradient determining behavior.

Returning now to the influence of the wish end of this gradient upon behavior, we have found reason to believe that the radiation of excitation from a wish or goal presentation is analogous to the induction phenomena of Pavlov. Is it possible that the radiation of excitation resulting from the state of hunger is also an induction phenomenon? This suggests questions as to the fundamental relations between "hunger" and "satisfaction" on the one hand, and excitation and inhibition on the other hand—questions which the present evidence does not permit us to answer with any certainty but which it will nevertheless be well to indicate.

We have already cited the well-known soporific effects of satiety. In most of Pavlov's experiments, however, internal inhibition arises under exactly the opposite conditions. Pavlov reports that every excitatory reflex tends sooner or later to become inhibitory. This replacement of an excitatory by an inhibitory process occurs, however, much more quickly if the excitatory reflex is not promptly

[25] Child, C. M.: Physiological Foundations of Behavior, 1924.
[26] Lashley, K. S.: Brain Mechanisms and Intelligence, 1929.

followed (reenforced) by the unconditioned stimulus and Pavlov also suggests that the explanation of this probably lies in the fact that the unconditioned stimulus puts an end to the excitement in the nervous elements corresponding to the conditioned reflex and that the excitatory process is probably much more prolonged in case the conditioned reflex fails to be reenforced.

The replacement of excitation by internal inhibition thus appears to be, as Pavlov indicates, a manifestation of fatigue—in direct contrast to the other type of inhibition corresponding to the soporific effects of satiety.

It would thus appear that inhibitory processes arise under two contrasting sets of conditions: (1) *as a reaction to satiety,** and (2) as a manifestation of fatigue following upon prolonged or intense excitement *resulting from an unsatisfied drive. The excitatory process* would then appear to be *an intermediate episode† between the inhibitory states of satiety and of fatigue.* We know from Pavlov's experiments that excitatory states tend to radiate. Is it possible that the phenomena of positive induction represent the next succeeding intermediate stage—in which excitation still continues to radiate from a center in which the (internal) inhibition of fatigue has already set in?

We may note in conclusion that the conditions that we here suggest as necessary for excitatory processes in general, are strikingly analogous to the conditions necessary for that optimum degree of excitation that makes possible the learning process: (1) *an unsatisfied drive or conflict* and (2) an intensity of that drive or conflict *that does not exceed a certain limit.*

But it is time now to wait for further experimental investigation of these fundamental relations and to turn to a more concrete problem and I should like to conclude this discussion with a few remarks about the mechanism of the therapeutic effect of psychoanalysis.

* Pavlov compares this inhibitory effect of the unconditioned stimulus to the relief from his duties of "an efficient and watchful signalman who after having performed his responsible duties has to be provided with an immediate rest during which he is refreshed so that he may afterwards perform his task again with the same efficiency as before."

† More specifically—the stimuli that give rise to excitatory processes appear in general to be of two sorts: (1) "hunger" from an unsatisfied drive and (2) "signal stimuli" that (we might say) "offer hope" of possible satisfaction (*i. e.*, if a person is not completely satiated, the sight of food alone arouses hunger).

Psychoanalysis started with the conception that psychoneuroses were the result of psychic traumata. Subsequent experience has indeed taught psychoanalysts not to picture the onset of a neurosis quite so dramatically as some of the early cases of hysteria might tempt us to do—but this modifies Freud's former views only to the extent that it leads us to expect in most cases not one great traumatic event but a series of lesser traumata.

Our discussion of the external reality function of the ego now puts us in a position to understand in physiological terms the nature of a psychic trauma. *A trauma is an experience calling forth a conflict of such intensity that there is no energy left free to be turned to neutral differential stimuli.* As a result of this, *the traumatic experience has been withdrawn from the learning process.*

The therapeutic functions of the analyst correspond to the two-fold conditions upon which depend the external reality function of the ego: (1) By various devices, but notably by interpretation of the symptoms, the analyst must reawaken the old conflicts and permit a reliving of the traumatic experiences; and (2) by judicious handling of the transference, he must when possible reduce the intensity of the conflict in its new edition so that energy may now be left free to be turned to neutral differential stimuli. Only in this way can the traumatic experience be made accessible to the learning process.

I shall conclude by calling attention to the confirmation which this gives to the views of Freud,[27] Ferenczi, Rank,[28] Alexander,[29] and others upon the importance of "recapitulation with insight" * as the essential therapeutic moment in psychoanalysis.

We may note also the light that the above considerations throw upon the well-known after-effect of a psychoanalytic treatment, an "after-effect" which is obviously a result of the learning process that has been made possible by the treatment.

[27] Freud, S.: (1) Further Recommendations in the Technique of Psychoanalysis. Recollection Repetition and Working Through. 1914. Collected Papers, Vol. II. (2) A General Introduction to Psychoanalysis, 1920. Chapter 28.

[28] Ferenczi, S. and Rank, O.: Entwicklungsziele der Psychoanalyse, 1924.

[29] Alexander, F.: A Metapsychological Description of the Process of Cure. International Journal of Psychoanalysis. 1923.

* The phrase is quoted from Alexander.

DISCUSSION

DR. C. MACFIE CAMPBELL (Boston, Mass.).—I suppose everyone here recognizes the difficulty of opening a discussion of this nature. Dr. French has dealt with two lines of investigation, two bodies of information which are highly technical. Each has its own idiom and skill with which he has talked in both of these idioms and carried one over to the other has given the ordinary clinical psychiatrist a sort of feeling of bewilderment; I felt that I was slowly slumbering after the speaker, trying to retranslate each of these idioms into more familiar terms.

The work of Pavlov is very fascinating. I had thought of it more as an analytic procedure, but Dr. French looks upon it more as a synthetic mode of approach. The early work on conditioned reflexes obviously comes very close to the work of each of us, who are dealing with the past experiences of our patients which have left the present types of reaction which we observe, and which would be unintelligible if we merely dealt with them in cross section without any study of their genesis.

In Freud's free association method we have a method extraordinarily fertile, by which we get an enormous amount of material dealing with the past experiences of the individual. It is our problem to see in what way these past experiences have laid the basis for the present type of reaction of the individual; or, in other words, how they have conditioned the individual to the experiences of his life.

When one takes up the general formulation of Pavlov's views and tries to apply them to the very complex field of psychopathology, one has the hesitation which the physiologist has when he tries to transfer to human neurology the data of experimental laboratories, even with regard to the simple reflexes, even with regard to such matters as posture and gait and the rôle of the various component parts of the central nervous system. He is very much on his guard against transferring the very accurate observations and deductions of the laboratory to the ward of the neurological hospital. We have to be on our guard against taking from a very simple experimental setting a certain association and immediately transferring it to a complex situation, unless we recognize that it is purely an analogy.

Rivers, in dealing with the sensory function of nerves, has differentiated between the protopathic and the epicritic function. You can pass from that up to the balance between other more primitive and more complex mechanisms and you can show that there is a protopathic tendency and an epicritic one, *e. g.*, the instincts versus the reason. You can talk in terms of epicritic and protopathic if you like but you have to do it with sufficient regard to the data observed at each special level.

I understand Pavlov with difficulty, in parts not at all. I do not know if we can accept all his statements with regard to inhibition and what it is. It seems to me a complex problem.

When we go beyond inhibition and when we find that we have sleep explained, and it is bracketed at once with hypnotism, then I cannot follow him.

When we pass on and find symptoms of dogs that are correlated directly with those of neurasthenic patients, then I again have very grave doubts.

When you listen to one of his papers, which I have heard only once in translation from the Russian, on the explanation of schizophrenia by the formulation of conditioned reflexes, then you may well protest that this is going too far and too rapidly. There are many factors to be taken into consideration in regard to catatonia and schizophrenia and neurasthenia which make the application of the conditioned reflex formulation at present premature and inadequate.

On the other hand, it is very important for us, working in this very complex field, to get what absolutely reliable and controlled data we can from those who are working in unusually favorable circumstances. The biochemists can study accurately the effect on the organism of the administration of carbon dioxide and other drugs. This may be a very great help to us in our understanding of such complex problems as stupor. So, the physiologist working with the conditioned reflexes may throw a great deal of light upon the problems which we have to deal with. But, we always have to keep in mind that the data he gives us have to be woven into this extraordinary complex situation; I do not think that more than the first steps have been taken to build a bridge between the work of the Pavlov school and the complicated psychological formulations of the Freudian school.

We know, of course, the general principle that every structure and every function is modified by experience. I suppose that the bridge over which each successive train passes is not the same as the one over which its predecessor passed. The dog that has been trained by Pavlov and the hysterical individual sensitized to a certain topic, both show the general principle that no experience leaves us unchanged. If we are going to work out the detailed application of that general principle I do not think we, as clinicians or as psychopathologists, dare tarry too long in the physiological laboratory or be too much influenced by the dictum of the physiological laboratory when it goes beyond the limited and somewhat abstract scope of the work there.

To bring extremely elaborate bodies of doctrine into association is very stimulating, but my great difficulty is in knowing whether we may not be dealing largely with analogy which, of course, has value as it may stimulate us and make our thought a little less narrow than it has been.

As to showing how the conditioned reflex theory is in relation to the therapeutic value of psychoanalysis, it reminds me of the gentleman who talked at the funeral. He hadn't known the departed but seeing he was on his feet and not having the benefit of knowing the departed he said he would at least like to say a word about Los Angeles.

Dr. Isador Coriat (Boston, Mass.).—I was very much interested in Dr. French's brilliant parallellisms because for several years I have had a similar interest. I had prepared for the International Psychological Congress in 1929 a paper on "Psychoanalysis and the Conditioned Reflexes," which has been unpublished.

I feel that this important and rather complicated problem can be understood only if we keep clearly in mind Freud's conceptions of the structure and the functions of the mental apparatus; that is, the division of the mental apparatus into the ego, the id and the superego, and also the formulation of the pregenital organization, particularly its deepest and earliest levels, that is, the oral nursing and the oral biting stages.

All these conditioned reflexes as worked out in Pavlov's laboratory have always appeared to me to be what we may term compulsive repetitions on the earlier oral erotic level from external stimuli rather than from the persistence of the earlier pregenital organization as we see it in the psychoneuroses. I refer particularly to salivary secretion in human beings due to manipulation of the oral cavity by dental instruments or local disease, or, in the pathological sense, in a subject which has engaged my attention for a number of years, the stammering neurosis, which is preëminently an oral neurosis of the pregenital period and in which the difficulties of speech are very frequently associated with profuse salivation.

The inhibition of the conditioned salivary reflex corresponds to repression in the psychoanalytic sense. As I recall some of Pavlov's experiments, when these reflexes are too strongly inhibited, or too strongly repressed there develops in the dogs what he terms a canine neurosis.

The success of the conditioned salivary reflex with one experimenter and its absolute failure with another experimenter under similar conditions finds, of course, its parallel in the psychoanalytic concept of resistance.

As I interpret these salivary reflexes, they appear to be regressions to this early pregenital level, that is, either externally conditioned, as in the dogs, or in human beings, the salivary reflex is internally conditioned and unconsciously motivated. Therefore, we have in Pavlov's experiments what may be called a comparative psychoanalysis which can be interpreted, in secondary physiological terms.

DR. GREGORY ZILBOORG (New York City).—May I rise to bring out a point of historical information rather than for a discussion of the paper. The whole problem of physiology versus psychology seems very frequently to resolve itself into the problem of organic versus functional. It is therefore extremely important to take note of what we learn by way of experiment, or at least by way of clinical observation on the one hand, and by way of the method of clear, logical thinking on the other, and thus try to correlate such experimental work as done by Pavlov with the observational work done by the clinical psychoanalysts.

The important thing, it seems to me, drawn by inference if not directly from Dr. French's paper, is that here we deal with two types of function and reaction which are viewed in their integrity. Also, that we deal here with an attempt to synthesize our observations or our conclusions with those of others and to synthesize them by way of approaching them from two apparently opposite poles.

In 1914 I was fortunate enough to be one of a small group of students who were experimented upon and who, at the same time, experimented in Bech-

terev's laboratory in Petrograd on conditioned reflexes. Bechterev, unfortunately, remained, both as a psychiatrist and a reflexologist almost unknown in this country. The small group of five men worked for a period of about a year, or a little longer, under various conditions producing within ourselves a series of conditioned reflexes that were later inhibited and then again released. We were excellent experimental dogs then and not psychoanalytic subjects.

Bechterev was the first to try to construct a bridge between Pavlov's purely physiological laboratory approach and Freud's purely clinical psychological intimate approach made in the psychoanalytical consultation room.

I do not believe that these two lines of investigation could be passed over very lightly. The integration of various functions within the individual whether they be responses to internal or external stimuli, the summation of which takes not only the lifetime of the individual but the lifetime of the race present a serious problem; this enormous and constant cumulative work cannot be passed over lightly by saying that physiology or the physiological laboratory cannot easily help the psychological clinican, or by stating that our conclusions are but analogies. It seems to me that there is something more than that. There is here an attempt to point out, regardless of structure and gross pathology, that while dealing with extremely complex functional units both in the physiological laboratory and in the clinic, we can yet reduce them to comparatively simple phenomena.

From the standpoint of scientific method the physicist makes his greatest contribution when he reduces the complex phenomenon to a comparatively simple formula in a simple set of simple phenomena. As long as the thing is complex he is unable to disclose its fundamental nature. Dr. French's work presents just this type of scientific simplification, a simplification which is not only of great theoretical but clinical value. One might mention incidentally the reflexological clinics that have been established in the course of the past six or eight years in Russia. The Bechterev trained psychiatrists, as you may know, became the official Soviet psychiatrists. There, in Russia, psychiatry is not "organic" or "Freudian"; it is Bolshevistic. But, whatever the historical and paradoxical justification of it might be, a lot of experimental work is done in this particular field on human beings, and I might mention in this connection the work that has been done with kleptomanic and other delinquent children.

I congratulate myself on having listened to Dr. French's paper for the second time. I congratulate Dr. French for a very bold, very definitely scientific method of thinking. I say "thinking" because I believe his paper in order to be completed, or his contentions in order to be verified requires a great deal of experimental work on dogs, a great deal of clinical investigation in psychoanalysis and a great deal of experimental work on human beings, such as Bechterev started in 1914 and as he, unfortunately, was unable to continue in view of the War and the Revolution.

DR. PAUL SCHILDER (New York City).—I do not know very much about the psychology of dogs, but I do think that the dogs have symbolic thinking. It happened in the course of some of Pavlov's experiments that there

was a flood in Petersburg, which upset the dogs and the conditioned reflexes in the dogs disappeared completely for awhile. After the conditioned reflexes had come back one of the laboratory attendants spilled water on the floor and again the conditioned reflexes disappeared for a short time. That is evidence of what I would call symbolic thinking.

Of course, we always think that the mental life of a dog is a very simple one. I do not think that we are justified in that. There is a very complicated psychic life in the dogs only we know very little about it. It is perhaps better to experiment a little in human beings who can tell us what is going on in them.

From this point of view I have experimented with human beings evoking conditioned reflexes in a way that light was followed by a strong electric current. The emphasis of these investigations was put on the question, What does the individual feel? What does the individual think concerning the conditioning? One sees then that psychic processes of an enormous complication go on in the short space of time between the flashing of the light and the withdrawing of the hand. There is the general attitude of the individual, the general expectation of the individual, as to what will happen. There is, at first, a general fear when the experiments are started, when the experiments are continued, the experience becomes more and more organized. The individual knows, although not in words, that the painful stimulus will come after a few seconds and he prepares himself to the stimulus which will follow until finally there is an organization of experience and this organization of experience is, of course, only valuable for a certain set of external factors and in some way the conditioned reflex is a difficulty of adapting one's self to a new situation, which is a very complicated psychic process.

We generally deceive ourselves and others in using the term reflex for a very complicated psychic attitude. We see, therefore, in human beings much more than we can see in dogs. We should study the conditioned reflexes more in human beings in order to know what is actually going on. The physiologist neglects the psychic sphere and studies only the actions, but it would be wrong to believe that these reactions are so simple.

When one studies the protocols of Pavlov one sees immediately that there are many things which do not fit. One day there is something wrong with the dog. The dogs have to be very carefully kept. There are moods in the dogs. They react to different experimenters in different ways. We have, therefore, very complicated psychological factors which we call reflexes without being actually entitled to do so.

DR. ADOLF MEYER (Baltimore, Md.).—It seems to me that I owe it both to the Pavlovian and to the analytical movements to say something on account of the fact that I have the privilege of being in immediate contact with work that is being carried on with both methods and in both senses.

I have asked myself, Why is it that we have not made more of the verbalization which has shown us this morning the convergence of kindred efforts to get at truth and at facts? Why have those tendencies not taken more shape with us than they actually have?

This may, of course, be due to the fact that on account of doing the work with concrete things one is perhaps not quite as strongly tempted to launch out in the direction—I don't want to call it thinking necessarily, but I may limit it to just thinking or at any rate formulation.

As a matter of fact, I think, as does Dr. Schilder, who has had some acquaintance with the working side by side of divergent and convergent methods as he has indicated, that we deal with very complex conditions, not merely analogies. As we verbalize, we keep it as much as possible on the ground of facts that are revealing themselves in investigations side by side.

There is in the paper by Dr. French a certain similarity to a paper which has not been mentioned but which deserves mention, not only for the similarity, but because it also refers to particular groups of facts, a paper by Brun in Zürich. He was a pupil of von Monakow, an excellent entomologist and psychoanalyst. He has given us a rather interesting study of the convergencies of biological conceptions and psychoanalytic conceptions. I must say that I like to hear the statement of convergencies of the reflexological and the Pavlovian and the psychoanalytic conceptions, but with the hint that lately, when I was under obligation to make some general statements in this field, I was very much tempted to characterize the last twenty-five years of our work as a period which might readily, at some future day, be historically called the wave of the mechanization of man.

It is important to get to simple terms and to simple investigations, but as Dr. Schilder has mentioned, and as Dr. Campbell has mentioned, we deal with facts that we want to cultivate side by side and that we do not want to substitute for each other too quickly. Let them be stimulations, each set of facts in its own way, and let us enjoy the convergencies which show in such discussions as we have had this morning.

Dr. A. Myerson (Boston, Mass.).—I was tempted to call for a bell-boy and ask him to page John B. Watson, Ivan Pavlov, and Sigmund Freud while Dr. French was reading his paper. I think Pavlov would have exploded; and what would have happened to Watson is scandalous to contemplate, since the whole of his behavioristic school is founded on the conditioned reflex. From what you read in Freud you may surmise this, that while he gives a polite bow now and then to organic and physiological matters, he would be scandalized by such an approchement made by one of his pupils, reading a paper of this kind.

As a matter of fact, the conditioned reflex is behavior of the simplest kind. It concerns what happens to the salivary secretion. The examples in everyday life are that fright dries up the salivary secretion and that any emotion may change the visceral functions, such as the gastrointestinal tract, the genitourinary tract, the lacteal supply, etc. When one studies the Pavlovian work factually, that is, independent of hypotheses, a simple reflex is modified by all kinds of environmental conditioning. No elaborate conduct is built up, and it is elaborate conduct that we are interested in. In other words, Pavlov has shown us how a simple reflex act may be brought into play by all kinds of artificial sitmuli, but he really shows us nothing else.

On the other hand, Freud shows, according to his followers, how reflex conduct is finally conditioned into very elaborate conduct by some unitary force —let us say, libido or something of that kind. Elaborate conduct is traced back to early beginnings, and in the case of some of Freud's followers to the fœtal stage itself. In other words, the fundamental Pavlovian work and the fundamental Freudian approach are diametric. They can only be brought together by clever and, to my mind, very ingenious efforts, such as that which we have heard in this present paper, which is very able, which is stimulating, but in my opinion, attempts what is at the present time impossible.

There is no reason for harmonizing the approach of Freud and that of Pavlov at the present time. They should stay in our techniques and in our discipline as diverse ways of approach to our problems. In the one case, we are dealing with simple conduct which can be actually and definitely measured, as for example, the number of drops of saliva which appear when one rings a bell. The other technique deals with interpretations, with symbols, with dream analysis, with free association, with divers techniques which are totally different from the kind of technique used by Pavlov.

I am loath to believe that these divers techniques, these diverse philosophies of approach can bring us results which are harmonious. That would be totally foreign to the experience of science.

DR. THOMAS M. FRENCH (Chicago, Ill.).—I think that the comments on my paper can be classified into two groups. The first set of comments take it for granted that it is a priori impossible to make any really useful correlation between two types of scientific approach that are so fundamentally different as those of Pavlov and Freud; in other words, that, whatever interest may attach to them, analogies such as I have been bringing forward, can only be regarded as amusing play. I am attempting, however, quite seriously, to make a detailed comparison of the work of the two schools and hope that there will be some who will be willing to examine my evidence in detail instead of rejecting it in advance as an attempt to solve the impossible.

The interrelations that I have worked out are, however, somewhat complicated and I think it is quite understandable that anyone who listens to them for the first time should be sceptical as to whether there has not been something left out in the comparison, whether I have not simply chosen the particular points that agree and neglect any notice of points that might conflict with my argument.

Of course, the only answer that I can give to that is to suggest that those who are interested actually look into the details of the work of both schools and find out whether or not there is conflicting evidence.

Turning to the second group of comments, I agree most heartily with Dr. Zilboorg's suggestion that these analogies should have further experimental confirmation. The usefulness of such a correlation as I have been attempting this morning can be none other than that of stimulating further experimental and clinical work in both fields in order to check up the correlations between them.

There is another set of comments to the effect that the phenomena of the conditioned reflexes in Pavlov's experiments are very much simpler than the phenomena that we observe in psychoanalysis and that we must add something to the physiological formulations before we can reach the psychological or psychoanalytic formulations.

I agree, also, with this. Unfortunately, time did not permit me to submit the whole of my paper. The second half of my paper attempts to go into considerable detail with reference to the subjective data of which Dr. Schilder speaks. No correlation between psychoanalysis and the work of Pavlov can, of course, be complete, that does not take into account the subjective data with which psychoanalysis deals preëminently but which is of course inaccessible to conditioned reflex experiments. It is of interest, however, to find that the phenomena of dreams and of other subjective states, that are reported to us only in speech—that these phenomena also admit of rather surprisingly simple explanations in the terms of Pavlov's data.

I must ask those who are interested in following this part of the comparison further, to read the more complete report which I hope to publish later.

NATURE OF FEEBLE-MINDEDNESS.

By ABRAHAM MYERSON, M. D., Boston.

This paper starts with the assumption that what we call feeble-mindedness is made up of a heterogeneous group of conditions which are only unified by one of many symptoms, viz., that they present an intelligence so low that it is declared pathological. Otherwise these conditions are entirely separate and have biological contacts with diseases and conditions not in themselves linked up with mental disease or defect. Thus the group of cretins biologically resembles the group of the organic brain diseases much less than it does other groups of diseases of the thyroid gland; and an imbecile, whose lowered mentality is due to a past encephalitis, has a very different biological background for his condition than his ward-mate whose mental disease we declare to be due to heredity.

Obviously, a study of the heredity of this heterogeneous group as a unit leads nowhere except into error and misunderstanding; and it becomes necessary, therefore, to study each group, differentiated well or poorly according to the present state of our knowledge, by itself, for its own hereditary relationships. We therefore divided up the feeble-minded at Waverley into groups. The classification was based on certain criteria differing in kind with each group, so that no unified scheme of separation was followed, because this is at present impossible.

1. The first group is that of the cretins, who look alike and have as the basic underlying defect a deficiency of the thyroid gland. This group is small in number but of great theoretic importance.

2. The second group is a larger one, also made up of individuals looking alike and presenting a unified group of characteristics, but whose etiology is at present unknown, although many interesting speculations have been made concerning them. This group is the so-called Mongolian imbecile, a name utterly wrong in that they are only superficially like Mongolians and that many of them are not imbeciles by our present classification.

3. The third group constitutes those individuals who present definite signs of organic disease of the central nervous system; pa-

ralyses, organic tremors, organic choreas and similar symptoms, the so-called spastic group, although this term should be discarded; and we shall consider them under the heading of the organic brain disease groups.

4. The fourth group constitutes the microcephalics, who, as is well known, are characterized by the extremely small head. As a matter of fact, this type of feeble-mindedness might better be included under the head of the organic states, since it is fundamentally organic.

5. The fifth group constitutes the so-called congenital syphilitics, a group concerning which there has very recently appeared considerable scepticism. This group is identified by a positive Wassermann reaction with or without signs of congenital syphilis. Whether or not these individuals owe their feeble-mindedness to syphilis is a matter which we shall discuss later, but for the time being they can be considered together under the heading, not of congenitally syphilitic feeble-minded but as, more certainly, the syphilitic feeble-minded, since the doubt in their case is whether or not the syphilis is the cause of the feeble-mindedness.

6. The last group is the most important because it is overwhelmingly the most numerous, and we have called it the unclassified group, meaning by this that in the present state of our knowledge we have no hint as to causation, although heredity has been largely blamed. Since heredity as an explanation or basis of classification is always open to suspicion I have avoided the use of this heading in our studies and use the term "unclassified" as synonymous with "unknown." Individuals of this group present a varied aspect. In many instances they show gross anomalies of appearance; in others no anomalies of any importance are present. They represent the group most commonly remaining in the community. There is, I believe, a disproportionate number of morons amongst them. In a general way they approach more nearly the diversity and the characteristics of the normal population, and they represent the most difficult social problem. After all is said and done, institutional care of the cretin, the Mongolian, the spastic type, the idiotic hydrocephalic, the grossly monkey-like type of the microcephalic presents no special difficulties and the patients arouse no other emotions than those of horror and pity. But among the unclassified we find the individuals who approach the normal in appearance and action;

we find persons who carry on in the community and who have access, so to speak, to copulation and reproduction; who pass amongst the mass as relatively normal. This unclassified group then probably represents our greatest problem and it is concerning this group that we have the least definite knowledge.

Such a series of groups does not by any means completely analyze the feeble-minded. It is perfectly obvious that among the unclassified are individuals with endocrine dysfunctions, the only difficulty being to state which endocrine gland is involved; a more important difficulty being that endocrine therapy is of relatively little value in these cases. There are other individuals not studied in this paper who represent the sense-defect type of feeble-mindedness: the deaf individuals, the blind idiot.

As a last part of this study, we have investigated certain outstanding family groups, families in which a large amount of feeble-mindedness has been found, several members of which are or were at the Walter E. Fernald School, and have taken special care to follow them into the community and to reach some conclusions as to the widespreadness of the familial defect. We cite a few of such families. The spectacular literature on feeble-mindedness concerns itself with the royal families of the feeble-minded, with the Jukes, the Kallikaks, the Nams, the Tribe of Ishmael, etc. These well-known studies seem to indicate that the collaterals of the widest range, as well as the immediate families themselves, are feeble-minded or defective, that an enormous amount of crime of the lower kind exists among such groups, that they are immensely prolific, that they threaten to swamp civilization. The specter of these families hangs over all the legislation which asks for the preventive measure of sterilization. Our family group studies of this type were undertaken to see whether or not we could find any such groups as the Nams, the Jukes, Kallikaks, etc.; whether or not our Waverley families * were so immensely prolific, and so fatally linked up with crime and widespreading feeble-mindedness.

I have eliminated as characters bearing on the genesis of feeble-mindedness such traits of the ancestors as headaches, fainting spells, tuberculosis, cancer, hemiplegia, alcoholism, organic brain disease in general, migraine, chorea, etc. Elsewhere[1] I have discussed the

* We have used the term "Waverley families" to indicate the families of those patients at the Walter E. Fernald School at Waverley, Mass.

non-relevance of these characters and the fact that the so-called polymorphic theory of heredity no longer applies. It was brought into the world at a time when clinical study had not even reached the stage of evolution of the discovery of the knee jerk; and represents a generalization possible only in the time of ignorance of that specific and scientific understanding which splits up unity into unities of varying and different types. I have regarded as hereditary characters of relevance to this research only feeble-mindedness of a low-grade intelligence, the existence of mental diseases of other types, and epilepsy. In other words, if an ancestor in the direct line is declared in the family history or in the interview to have been peculiar and suffered from headaches, I have not regarded this type of fact as explanatory of the existence of cretinism or other form of feeble-mindedness. I have not been impressed with the fact that social workers, investigating cases, discover marital difficulty, in view of the fact that one out of every seven marriages in the United States ends in divorce and at least as many are unhappy in the marital bondage. Elsewhere[1] I have also given my reasons for not considering alcoholism as a factor. I regard alcoholism either as a definite social phenomenon or else as representing a pharmacological idiosyncrasy which may have psychopathological ramifications. But these are not the ramifications of mental disease.

Similarly with organic brain disease.[1] In the etiology of feeble-mindedness the occurrence of shock, paralysis, etc., in the ancestors has in many studies been given an importance which it does not in the least deserve. The bulk of "shock" and "paralysis" is due to arteriosclerosis of the brain and is part of the arterial degeneration which takes place with middle and old age, although it occurs now and then in young individuals. However, all persons acquire cerebral arteriosclerosis if they live long enough, and, in a sense, the greater incidence of shock and paralysis in the ancestors of a group is evidence for the longevity of that group rather than a mark of degeneration. This is quite adequately borne out by the statistics of Diem, Jolly, and others, and in fact whatever data we have on the matter show that organic brain disease, as such, occurs more frequently in the ancestors of the normal than in the ancestors of the insane and feeble-minded.

Section I.

A. Concerning the heredity of the sporadic cretin, the Mongolian, the organic brain disease cases, elsewhere [3] I have given in detail the statistics of these groups and discussed the literature concerning them. I refer the reader to this publication for a comprehensive survey. It appears in this study that no known hereditary basis can explain the incidence of cretinism, mongolism and the various types of feeble-minded associated with organic brain disease. True, in the institutions it will appear that many of the ancestors were intemperate, a few had criminal records; there was considerable physical illness and much matrimonial difficulty in the brothers and parents, the uncles and aunts of the patients.

If one placed all these things into a statistical hopper and gave them the same set of meanings and values, one could easily disclose a strong hereditary taint in the case of the feeble-minded individuals studied, and prove, for example, that an infection or birth injury merely brought out a neuropathic taint. This has been extensively done in the past and is fundamentally erroneous. Matrimonial difficulty and general physical illness are altogether too common to be given specific hereditary value. Social delinquency cannot be placed in the same class as infectious diseases. By extending the list of conditions which may be declared hereditary, one finally extinguishes all differences, comes down to the fact that man is mortal, has weaknesses and is sinful. A linking-up can thus be made between the vicissitudes and misfortunes of life and feeble-mindedness. *The important facts in this study of these groups are, so far as heredity is concerned, the relative absence of feeble-mindedness in brothers and sisters, and the relatively few cases where definite feeble-mindedness occurred in the parents.*

B. A group which we have studied in great detail is the so-called syphilitic group, to determine whether or not syphilis was an important factor in producing feeble-mindedness and whether there was any evidence that hereditary factors were more important. It has been generally accepted in the past that syphilis, in addition to its effects on the individual, is an agent in the creation of race deterioration. Its effect in bringing about miscarriage, abortion, and stillbirth is so well established that it is an easy transition from the facts relating to these matters to the opinion that it is responsible

for the damage which manifests itself in inferior and defective individuals. The existence of congenital syphilis is, of course, a matter concerning which there can be no possible controversy.

In recent years a good deal of literature has appeared which very successfully challenges the opinion that syphilis causes feeble-mindedness. Weiss and Isgur,[3] the Solomons,[4] the later writings of Haines[5] and especially the very comprehensive study of the subject done by Neil Dayton,[6] have not corroborated the opinion that syphilis causes feeble-mindedness. The study of our 59 cases, designated as congenital syphilitics, together with a study of the entire literature led me to the following conclusions:

1. A survey of the literature * shows that the incidence of syphilis among the inmates of institutions for the feeble-minded is slightly, if at all, greater than that of non-defective children.

2. Intensive study made of the inmates of the Walter E. Fernald School for the Feeble-minded shows that there are few signs of neuro-syphilis among those labeled as congenital syphilitic feeble-minded. In other words, direct evidence of the brain being involved is scanty.

3. *A very large proportion of this group of feeble-minded comes from families in which there is much feeble-mindedness; in other words, if there is a hereditary element in feeble-mindedness, it may be held responsible for a large proportion of the cases labelled congenital syphilitic feeble-minded.*

4. The social stratum and the way of life of the parents of these feeble-minded would lead one logically to expect a large percentage of syphilis amongst their descendants, irrespective of the feeble-mindedness.

5. While it cannot be doubted that syphilis causes alterations of the brain, and thus produces *dementia* both in the adult and in the child, there is no proof at present that syphilis causes *feeble-mindedness*. There is nothing in the general appearance and the general reactions of the congenital syphilitic feeble-minded to distinguish them from the mass of the feeble-minded.† Therefore,

* In addition to Dayton's work, Haines, Weiss and Isgur, and Solomon have contributed statistics which corroborate the above.

† I realize that some hospital physicians declare that there are peculiarities in the conduct and general psychology of the congenitally syphilitic feeble-minded. I am quite sure if they were mixed up with other groups, it would be impossible for the most astute clinician to single them out.

there is ground for the assertion that, at the present time, we can safely state that syphilis of ancestors causes little, if any, feeble-mindedness in descendants.

Section II.—Unclassified Group.

General physical inferiority in the unclassified group.

Of some 535 of this group closely studied, there were 24 individuals so far below the average height that the term "dwarf" could be legitimately applied to them; for example, adult males who ranged under 4 feet 8 inches in height and adult females who ranged under 4 feet 4 inches in height. There were 17 individuals who were blind, 8 individuals who were deaf, 21 persons who showed the less obvious signs of organic brain disease, such as difference in reflexes, organic pupillary signs, etc., but who could not be further classified or linked up with any known cause or infection. There were 125 persons whose general physique presented so many signs of inferiority as to be classed as "general inferior individuals." This term needs a little amplification. We did not include many things which are ordinarily regarded as signs of inferiority, and which have so been classified by most writers, as, for example, very poor posture, very bad teeth, strabismus, etc. We excluded strabismus because we did not know what its incidence in the general population is. We excluded poor posture because that is merely often a reflex of the mental state itself. Very few really feeble-minded individuals have good posture. The slouchy let-down that comes with a lowered mentality is very conspicuous, but it can be imitated by any one with a mimetic sense. By "general inferiority" we meant those individuals who are small in size, who showed bony and other deformities, who are markedly asymmetrical—who would be conspicuous, in a word, anywhere, by the actual physical defect manifested in their skeletal and head asymmetry and their inferior size.

Thirty-three individuals had marked vasomotor disturbance, as manifested by cyanosed hands and feet. There were 44 persons who had marked cranial deformity, not approaching the true acercephalus, or tower head, and excluding, of course, the microcephalics and the hydrocephalics. It will thus be seen that physical inferiority is a very conspicuous accompaniment of feeble-mindedness of our institutional group.

I divided our feeble-minded into three groups: those of the lowest level up to the age of three or to a corresponding I. Q.; those of a middle level up to a mental age of six or a corresponding I. Q.; and those of a mental age of six upward, with very few ranging above nine. *I believe that this is a better classification than the common one of idiots, imbeciles, and morons.* First of all, it is a more convenient one, and second, it is more logical, since the three types of feeble-mindedness are not sharply separable but really flow into a continuum of mental defect. Why give three different Greek names to different degrees of feeble-mindedness?

These three types of mentality in the unclassified group were studied in relationship to the incidence of bodily defect, excluding from this consideration the dwarfs, the blind, the deaf, the microcephalics, and the hydrocephalics, and considering only the relationship of general inferiority as previously defined. Our table also includes the relationship of what we call negative physical examination in relationship to these three degrees of mental state. By negative physical examination, we merely meant no outstanding physical defect. Poor feet, poor posture, and the like, if not associated with any other defect, were classified as negative. The following table shows the results:

	General inferiority.		Negative.		
	Expected number.	Actual number.	Expected number.	Actual number.	Total.
Lowest level	14	25	38	36	62
Middle level	36	44	96	95	152
Highest level	39	22	104	109	165
					379

It will be seen that general bodily inferiority amongst the lowest mentalities was nearly twice as high as expected, the amount in the middle grades was slightly more than expected, and in the highest mentalities, it was a little more than half as much as was expected. It is curious to note that the expected number of negatives in this group was closely approached by the actual number of negatives found. Thus, general inferiority in this group is very closely related to the degree of mentality. At the same time, the rather contradictory finding is that negative physical states appear equally among all three mental groups.

Potter, cited by Davies,[1] states that the hereditary cases were under-developed physically, as well as retarded mentally, while

the non-hereditary cases showed only a mental retardation. This is not our experience. We did not, unfortunately, make a statistical study of this point, but we have records of many cases with marked physical defect, some to the point of grotesque defectiveness, amongst the cases with negative family histories. Nor do we find that the hereditary cases belong to any special level of the feeble-minded. It is true that idiots do not beget idiots, because idiots do not propagate. They are shut off from propagation by the severity of their defects, and so are excluded from further damaging the race. But there are high-grade morons, low-grade morons, and imbeciles in our family cases. It is our impression, however, that the morons predominate in the hereditary cases.

An interesting confirmation of this observation on the physical defectiveness of the feeble-minded comes from M. W. Cliff:[8]

> The results of our observations in the feeble-minded have yielded little on which to base a theory as to basic factors involved. There has been much theorizing in this particular field, and the author felt that he would not be criticized if he declined to draw other general conclusions. The constant association of structural development with mental defect and mental impairment is a condition so obvious as to argue a more or less common basis. Further than this, it is impossible to go at the present time.
>
> To summarize our findings in general terms, we would state that the Roentgen examination of the feeble-minded shows a very high percentage of malformation and developmental defect of the skull, and in those cases associated with endocrine disturbance, there are, as well, marked changes in the process of bone development, and that recognizable changes in the bony structure of the sella turcica are relatively uncommon even in those conditions in which the pituitary gland is involved.

Neil Dayton,[9] in his excellent survey of 3353 retarded school children in Massachusetts, states that groups with the lower intelligence quotient tend to be under average height and weight. Groups with the higher intelligence quotient, that is, the higher morons, tend to approach the normal population in height and weight. He states, amongst other conclusions, that there is an apparent relationship between (1) height and intelligence, (2) weight and intelligence. The over-height and the over-weight groups have relatively more patients in the higher intelligence levels than the under-height and the under-weight groups. And furthermore, in the lower levels of intelligence, there are more children under average in height and in weight. In a more comprehensive paper, entitled, " Survey of Retarded Children in the Public Schools of

Massachusetts," Dayton states: "However, when we consider the physical deficiencies, we find a larger number than even the most pessimistic portion of the 'legend' has ever outlined. Gastrointestinal defect, chiefly teeth and tonsils, easily takes the lead." (It will be remembered that we have excluded gastrointestinal defect in our consideration of the defectiveness of the feeble-minded.) "The next in order of importance is stigmata. Just what stigmata may mean has always been more or less of a question, as their exact relationship with mental defect has not been satisfactorily outlined. Defects of special senses come next, and are followed by circulatory, neurological, and skeletal defects.

"In general, the physical defects outline themselves as follows:

> "4 out of 5 have defect of teeth or tonsils.
> 1 out of 2 shows stigmata.
> 1 out of 4 has defective vision or hearing.
> 1 out of 6 has a defective heart or defective circulation
> 1 out of 7 has a neurologic defect or disease.
> 1 out of 7 has a skeletal defect."

When it is taken into account that in our personal studies we have excluded such defects as poor teeth, poor tonsils, the ordinary stigmata, such as defective ears, a defective palate, or a defective heart, which are tremendously common amongst our patients, then it becomes obvious that the amount of physical defect amongst the feeble-minded is appalling. On the whole, it may safely be stated as a general proposition, that the feeble-minded are feeble-bodied. When one takes into account the cretins, the mongols, and the organic brain diseases as well, then the picture becomes entirely organic and the bodily defectiveness is as conspicuous or more so than the mental defectiveness. But even in the unclassified, whose etiology is not definitely known, who present no extraordinary mark of bodily disease, the picture of general defectiveness is the correct picture of the situation. That is, while the mental defect and the physical defect do not run necessarily parallel, there is, nevertheless, a tremendous load of physical defect amongst the feeble-minded which makes it possible to state that in a large number of institutional and non-institutional cases, the whole organism is probably defective. It is results like this which make it impossible to conceive of feeble-mindedness as a Mendelian character.

SOME SELECTED FAMILY GROUPS.

I have selected from the hundreds of families available for detailed exposition a number of family groups. Some represent what we call our "worst families." They were selected to show that even in these "worst families," collateral groups remain normal, and there does not seem to be anything like the widespread degeneracy, crime, etc., of the famous royal families of feeble-mindedness, the Nams, the Kallikaks, the Jukes, etc. In a previous publication [1] I have criticized the type of study represented by the Kallikak family as non-representative of feeble-mindedness; and indeed I am extremely doubtful that they really represent feeble-mindedness at all, for part of the picture is low cultural level and a good deal of the rest may be due to the fact that when these studies were made, normal intelligence was placed far too high. Our studies do not lead to the same extreme pessimism and fear for the race which permeate the classical family studies. In most cases the main contribution of a family and its collateral groups has been normal, even though several members have been low grade. In my opinion, there has been too much pointing of alarm and too little sober investigation for the proper understanding of the problem of feeble-mindedness. I cannot resist making this final statement: that the statistical necessities imposed by Mendelian ratios have not helped the situation at all.

I here cite a series of ten families, selected from the several hundred families studied, as representative of the varying family situations in which feeble-mindedness appears.

The first family (Chart I) may be called one of our "worst" families in that we have definite institutional records of two generations with evidence of feeble-mindedness in three generations. The above chart which represents four generations, shows a relatively enormous amount of feeble-mindedness arising from the union of two feeble-minded individuals of the second generation, with 11 institutionalized individuals in the third and fourth generation. Nevertheless even here, certain of the collateral branches are normal so far as can be discovered; and even in the direct line, as in the fourth generation, normal individuals appear. This group has not, so far as can be seen, had a tremendous progeny. While there has been considerable minor delinquency, no real social damage, except the expense of maintaining the feeble-minded mem-

bers, has appeared. In other words, two of the popular bogeys concerning the feeble-minded, their enormous fecundity and their tremendous participation in crime, are certainly not justified by the study of this family.

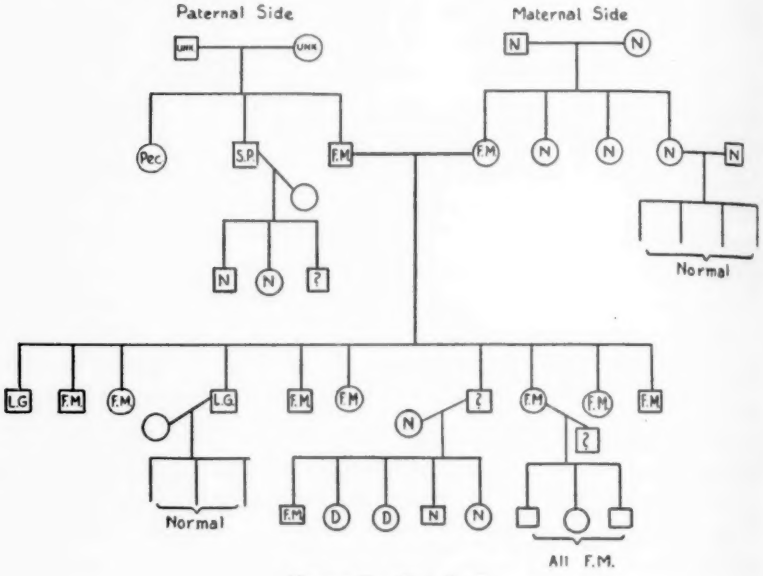

CHART I.—Family I.

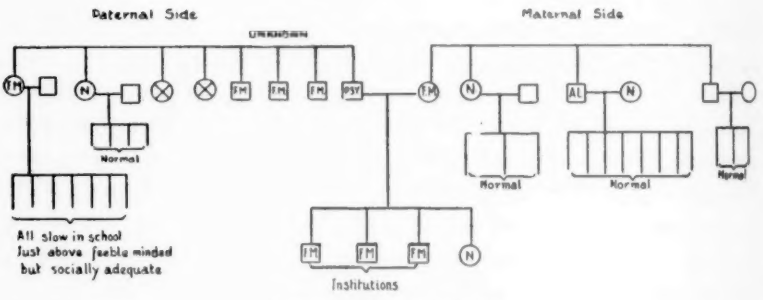

CHART II.—Family II.

The second family (Chart II) which represents one with more than average involvement of feeble-mindedness, is depicted in the second chart. There are three brothers of this group in Massachusetts institutions for the feeble-minded. It will be seen by reference

to the chart that there was considerable feeble-mindedness in the brothers of the father of these individuals and that the mother was feeble-minded. Nevertheless the collateral branches have on the whole done well. A eugenical segregation has been accomplished to some extent at least, by state hospital incarceration at an early age.

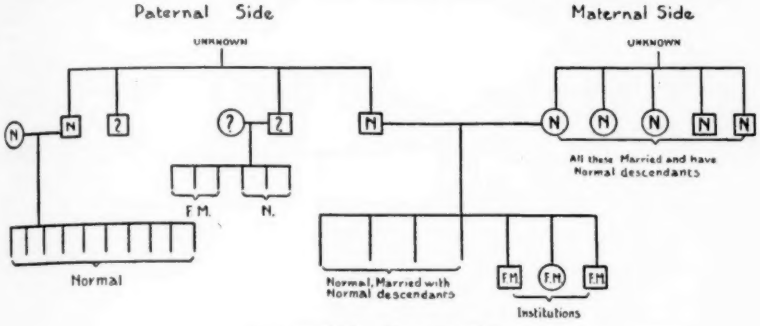

CHART III.—Family III.

Family III (Chart III) centers around three individuals at the state schools for the feeble-minded in Massachusetts, who are brothers and sisters. It will be seen that though there was a very large incidence of feeble-minded, the family has contributed normal

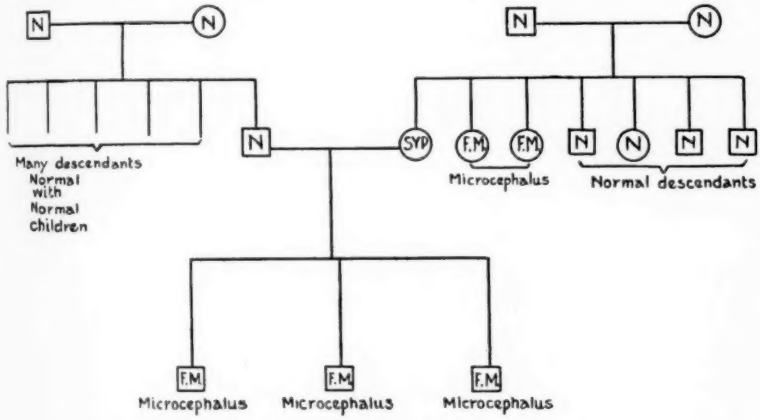

CHART IV.—Family IV.

descendants even in the worst groups. There is no record in this case indicating in the least that there was any conspicuous social disorder created by this group.

Family IV (Chart IV) illustrates two generations of microcephalus in which there are four institutional cases. As far as we

can learn, the collateral branches are normal and there is no explanation of the microcephalus except an hereditary process. The syphilis of the mother of one group cannot be evoked as an explanation. All attempts to link up syphilis with microcephalus have fundamentally failed; and in this case, all tests evolved on the individuals have been negative.

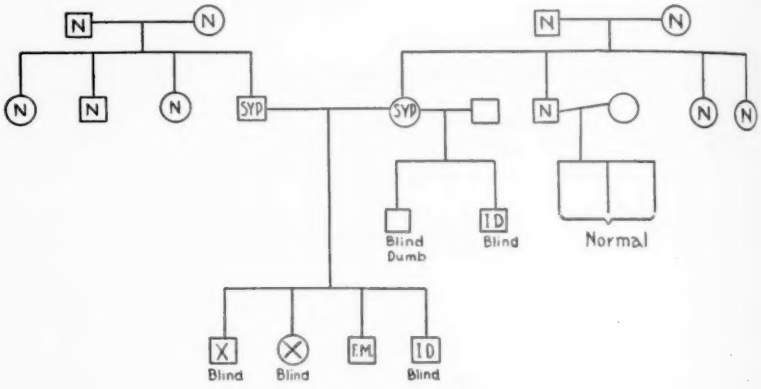

CHART V.—Family V.

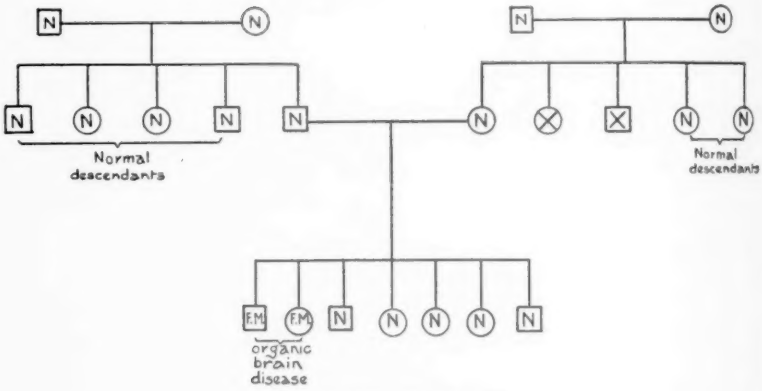

CHART VI.—Family VI.

Family V (Chart V) is interesting because of the incidence of blindness and feeble-mindedness together with the incidence of syphilis in the parents of these children.

Family VI (Chart VI) represents the unaccountable appearance of two siblings both of whom are feeble-minded and who have

organic brain and central nervous system disease. The rest of the siblings are normal and the family history on the whole is good. No explanation presents itself except abiogenesis.

Family VII (Chart VII) is of great interest because the familial background is entirely normal. There occurred in the present hospital generation, 13 children of whom six died shortly after birth.

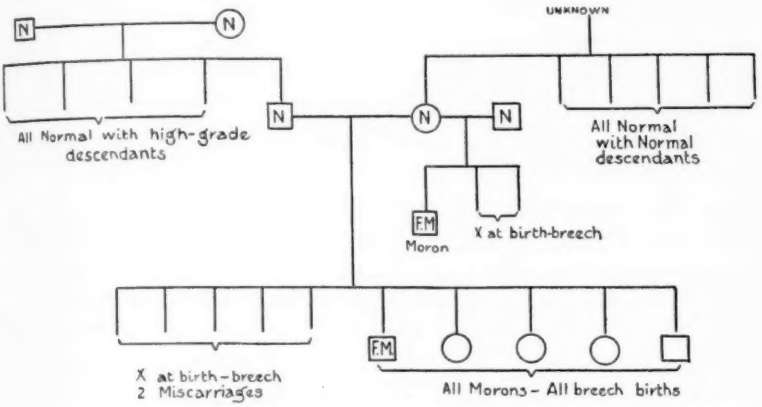

CHART VII.—Family VII.

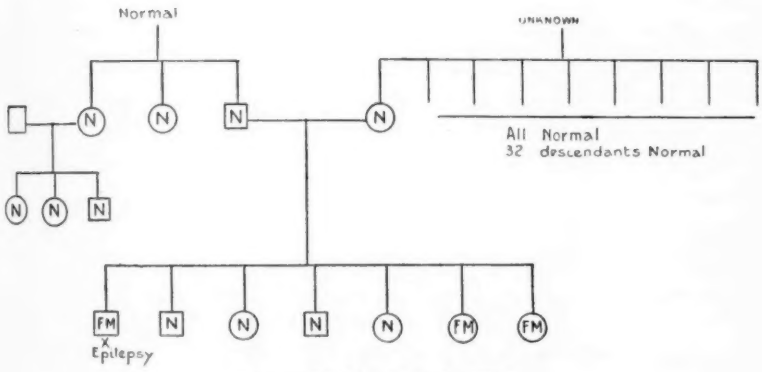

CHART VIII.—Family VIII.

There were two miscarriages and the remaining five are all morons. All of them have been examined. The outstanding feature is this: that all of the 13 children were difficult breech births.

Family VIII (Chart VIII) is cited because there are three defective children with five normal in the hospital generation. The

parents, the grandparents, the collaterals would compare favorably with similar groups in the average family in the community. There has been no delinquency, no alcoholism and no pauperism. Such cases as the above can be multiplied many times in our records.

Families IX and X are cited because they represent far the more important groups of the feeble-minded in which the patient is the

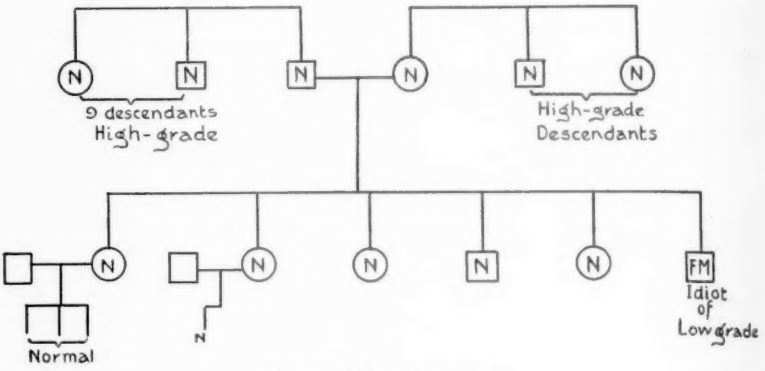

CHART IX.—Family IX.

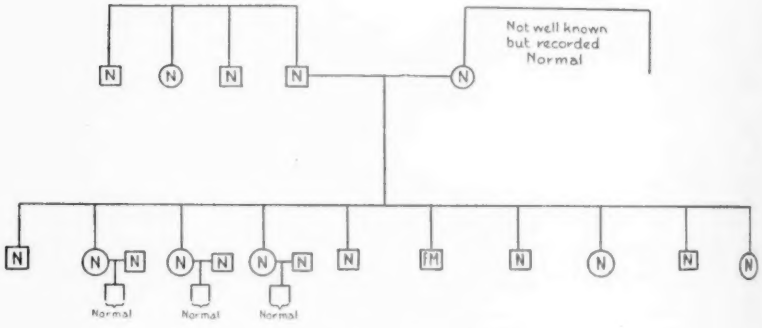

CHART X.—Family X.

only feeble-minded member of otherwise normal families. These have not been cited enough in the literature.

Family IX (Chart IX) is such a case. He had some illness early in his childhood. He is an idiot of the lowest grade. His brothers, sisters, nieces and nephews are normal.

Family X (Chart X): The hospital patient had some illness early in childhood, became backward, and is now an imbecile.

There are 11 adult brothers and sisters so that a sufficient length of time has elapsed to discover whether or not other diseases developed in this family. The nieces, nephews as well as the brothers and sisters are all normal. In these two cases we have families whose records indicate adequacy in every way.

HEREDITY OF THE UNCLASSIFIED GROUP.

I refer the reader to the extensive analysis of this important matter made by Dr. Elkind in a study of my cases in the Bulletin of the Massachusetts Department of Mental Diseases for April 1930 (pp. 200-202 with Tables XVII and XVIII which are here reproduced as Tables I and II). The conclusion of Dr. Elkind is that in so far as this material went, the inheritance of the feeble-minded in the unclassified group behaved more or less as a simple recessive of the Mendelian type. My reasons for doubting this conclusion will appear, I think, throughout the previous discussion. Furthermore, a study made by Dayton[*] of the feeble-minded groups outside of the institutions shows less than half as much feeble-mindedness in the ancestors and siblings as was found in our group. In other words, we have to assume that there is some biological difference between institutionalized cases and non-institutionalized cases, in order to accept both conclusions. Furthermore, some excellent work done by the sociologists, Freeman, Holzinger and Mitchell,[2] shows that the children of two feeble-minded parents, if transferred early enough from the environment which would be granted them by their real parents to a better environment offered them by foster parents of normal type, reach a mental level, as indicated by the psychological tests, of distinctly higher degree than can be expected of the feeble-minded; that while they do not reach a good normal, they reach a mental status superior to that which would result if intelligence were a Mendelian character and not a potentiality, developed and evolved by environment.

Indeed the great value of the work done by these writers lies in the fact that they have shown that children transferred early from one environment to another tend on the whole to resemble their foster brothers and sisters in intelligence more than they do their own brothers and sisters; and since they usually pass from a low grade parental environment to a higher grade foster environment, pass upward in the intelligence scale. Indeed, if intelligence,

TABLE I.

DISTRIBUTION BY NUMBER OF FEEBLE-MINDED SIBLINGS (INCLUDING PATIENT) AND PARENTAL HEREDITY OF PATIENTS ADMITTED FOR THE FIRST TIME TO WRENTHAM STATE SCHOOL 1920-1925, INCLUSIVE.

PATIENTS WITH STATED PARENTAL HEREDITY.

Number of F. M. siblings.	Total.			Both normal.			One feeble-minded. One normal.			Both feeble-minded.			One feeble-minded. One mental.			One feeble-minded. One epileptic.			Both mental.			One epileptic. One mental.			One epileptic. One normal.			One both feeble-minded and mental.			One mental. One unknown.			One feeble-minded. One unknown.			Unknown.		
	Both.	Males.	Females.	Both.	Males.	Females.	Both.	Males.	Females.	Both.	Males.	Females.	Both.	Males.	Females.	Both.	Males.	Females.	Both.	Males.	Females.	Both.	Males.	Females.	Both.	Males.	Females.	Both.	Males.	Females.	Both.	Males.	Females.	Both.	Males.	Females.	Both.	Males.	Females.
Total	983	417	566	346	183	163	26	15	11	49	17	32	11	3	8	3	1	2	3	3	0	2	1	1	15	9	6	1	1	0	34	19	15	77	13	64	416	152	264
1	490	243	247	312	166	146	7	4	3	2	1	1	4	1	3	1	0	1	1	1	0	1	0	1	12	7	5	0	0	0	12	9	3	22	6	16	116	48	68
2	143	50	93	13	7	6	9	3	6	15	5	10	2	0	2	1	1	0	2	2	0	0	0	0	3	2	1	1	1	0	6	2	4	20	1	19	71	26	45
3	45	16	29	1	0	1	1	1	0	13	4	9	3	2	1	0	0	0	0	0	0	1	1	0	0	0	0	0	0	0	2	1	1	8	2	6	16	5	11
4	18	7	11	1	1	0	0	0	0	5	2	3	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1	0	2	0	2	9	3	6
5	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
6	3	1	2	0	0	0	1	1	0	2	0	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
7	2	0	2	0	0	0	0	0	0	2	0	2	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
8	1	0	1	0	0	0	0	0	0	1	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Unknown ..	281	100	181	19	9	10	8	6	2	9	5	4	2	0	2	1	0	1	0	0	0	0	0	0	0	0	0	0	0	0	13	6	7	25	4	21	204	70	134

TABLE II.

TABLE II.

NUMBER OF FAMILIES OF PATIENTS ADMITTED FOR THE FIRST TIME TO WRENTHAM STATE SCHOOL HAVING SPECIFIED NUMBER OF SIBLINGS (INCLUDING PATIENT) WITH PERCENTAGE OF FEEBLE-MINDED CHILDREN AMONG THEM—1920-1925, INCLUSIVE, WITH THE EXPECTED PERCENTAGES IF FEEBLE-MINDEDNESS BEHAVES AS A SIMPLE RECESSIVE.

	Total families.	FAMILIES HAVING SPECIFIED NUMBER OF SIBLINGS (INCLUDING PATIENT).																			
		1		2		3		4		5		6		7		8		9		10	
		Number.	Per cent of feeble-minded.	Number.	Per cent of feeble-minded.	Number.	Per cent of feeble-minded.	Number.	Per cent of feeble-minded.	Number.	Per cent of feeble-minded.	Number.	Per cent of feeble-minded.	Number.	Per cent of feeble-minded.	Number.	Per cent of feeble-minded.	Number.	Per cent of feeble-minded.	Number.	Per cent of feeble-minded.
Observed	597 *	76	100	97	60	115	47	115	37	82	30	25	23	5	30	25	23	12	14	39	16
Expected	... †	..	100	..	57±2	...	43±2	...	37±2	..	33±2	..	30±3	..	29±6	..	28±3	..	27±4	..	26±1

* 591 out of 983 admissions (about 60 per cent) had information as to size of family and number of feeble-minded siblings. We have here discarded "unknowns" on the assumption that, were the information known instead of unknown, the meaning of the table would not be materially changed. We have assumed that the probability of obtaining information as to the number of feeble-minded siblings is not correlated with the size of the family; also that where the number of feeble-minded siblings is known and nothing is stated about the children who are not declared feeble-minded, they are not feeble-minded. In addition patients have been counted as representing separate families; that is, a family has been counted twice, or in rare instances three times. It is not uncommon in schools for the feeble-minded to have a number of related siblings under care. This particular circumstance lessens somewhat the statistical significance one would attach to the above percentages.

The observed distribution includes both the classified and the unclassified. When the unclassified cases were analyzed by themselves, the same observed ratios were noted as were found for the group as a whole. Whatever differences occur were found to be not significant statistically.

† The expected percentages in Table II were estimated as follows: Let p = the chance of a feeble-minded person among siblings in the family, n = number of siblings in a family, N = number of families. In the general population then by chance alone we should expect the siblings of the family to be distributed according to the terms of the expansion of $(p+q)^n$. But the above data were selected obviously so that they contained no families in which no siblings were feebleminded.

Therefore $\Sigma(p+q)^n$ in the selected sample is $1-1q^n$ instead of unity. The mean of the sample is therefore $\frac{np}{1-q^n}$ and the expected proportion of feeble-minded in the family of n siblings is $\frac{p}{1-q^n} = P$. The standard deviation of the above distribution is

$$\sigma = \sqrt{\frac{npq + n^2p^2}{1-q^n} - \frac{n^2p^2}{(1-p^n)^2}}$$

and the s. d. of the proportion P is

$$\sigma P = \sqrt{\frac{pq}{n(1-q^n)} + pP - P^2} = \sqrt{P\left(\frac{q}{n} + p - q\right)}.$$

The mean proportion of N samples of n siblings is $\frac{\Sigma P}{N}$ and the s. d. of the mean $\frac{\Sigma P}{N}$ is $\frac{\sigma P}{\sqrt{N}}$.

Some estimate of the p must be made in order to evaluate these expressions. If we assume that feeble-mindedness is a character which behaves as a Mendelian recessive then we can let $p = \frac{1}{4}$. On this assumption the above expected proportions with their standard deviations have been computed. The agreement of the observed percentages with the expected percentages is very good except for the families of 9 and 10 siblings where the number of feeble-minded observed is significantly less than that which is theoretically expected.

which after all comprises many things, is developed by use, imitation, example and training, this would naturally be expected.

It may be concluded in regard to this unclassified group, which I repeat is not a biological entity but merely represents the broad domains of our ignorance in the matter, that low grade mentality of the type graded as feeble-minded runs in family groups, and probably rests in a certain measure upon the qualities of the germ plasm and is transmitted from generation to generation. That it is Mendelian in any sense in which that term is used today, seems to me exceedingly unlikely and I do not believe any amount of statistical analyses can solve the problem. Only experiment can do this by separating out the factors by which what we call intelligence and conduct, express themselves in the social scheme. At no point is nature really separated from nurture. The environment, in its intricate forms, flows in and out of the individual, developing, inhibiting, exciting, twisting and thwarting the mental potentialities exactly as it does in the case of the physical qualities of living creatures.

I suggest to the great foundations that they stop subsidizing researches which tell us the number of feeble-minded found in any community or town, that they no longer subsidize the kind of statistical gossip which at present passes as one of the phases of eugenics. Nothing is gained by the multiplication of uncertain researches, except, perhaps, to keep alive the interest in the subject. Let us subsidize instead the removal as soon as born of children who come into low-grade families, perhaps in families where the advent of the new child is a great burden and where the parents will willingly give it up for the sake of having it adequately cared for. Let the children thus experimented upon be removed to high-grade homes or placed under high-grade environmental conditions. A number of children sufficient for such a study could easily be determined by the statisticians. Then let good nutrition from the earliest days, good example and devoted education do their best. I believe that such an experiment would lessen the talk about heredity and increase the evaluation of the environment in the development of the intelligence, the character and the personality of human beings. It would be shown, I believe, what must already be apparent to those who have followed the later development of biological science: That germ plasm is a living tissue imbedded in a mass of living tissue

with which it constantly interacts; that the whole organism is a bit of organic matter embedded in a huge environment which continually influences it, making for good or for ill; that these environmental forces reach through the bodily tissues and into the germ plasm, disturbing its stability, capable of destroying it, injuring it and of improving it. It seems almost presumptuous to think that the tiny mass of germ plasm of each individual is an independent entity, since all the germ plasm in the world in its sum total compares with the mass of the environment as a drop of water to the Atlantic Ocean.

I offer as a working hypothesis for psychiatry and for biological medicine the concept that the most practical working field for us is the influence of environment upon germ plasm, and upon the mind and the personality of the human being. This environment needs to be studied in its minutest phases; uterine conditions ought to be a field for research,* and the vaginal smear method of Stockard and Papanicoloau offers at least a hint as to how this research can be conducted. A record of intra-uterine conditions is possible; at least a more careful study of such conditions can be undertaken. Within this environment the earliest and most profound influences are exerted upon the developing individual, and yet almost no research has been carried on in this field. A careful study of nutrition before pregnancy, of the effects of infectious diseases upon ovulation needs to be instituted. It is quite possible that a defective individual occurring in a normal family may represent not a hidden Mendelian character of altogether hypothetical origin, but the effect of some passing infection upon germ plasm. The influences of the crowding of the slums, under-nutrition, infectious diseases, work in factories, and poor hygienic conditions is a more profitable study, in my opinion and according to this working hypothesis, than studies made of defective communities existing under traditions and actual physical environments which might in themselves explain in some measure their defectiveness. While alcohol and syphilis may not be the blastophthoric forces which injure germ plasm, the case certainly is not closed against their effects, and we need not confine ourselves to the mere physical con-

*See in this connection the references in the Bulletin of the Massachusetts Department of Mental Diseases, April, 1930, p. 229, for a survey of the literature on the modifications of heredity by environmental forces.

ditions which surround groups of the population in an effort to discover the blastophthoric forces. The day is past when emotional states can be dismissed as merely mental. We know that a great emotion is a bodily event reaching through every tissue and changing the chemistry of the entire organism in some measure. In this field, as in every field in which the living creature is studied, the sum total of the forces operating on the human being must be taken into account. Heredity is the name given to an interior force, or set of forces, which operates fairly stably but is constantly influenced by an immense number of other forces. To explain the defective individual by this inner force alone is to leave out of account the rest of life in all its varied and potent aspects.

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BLOOD CHOLESTEROL STUDIES IN MENTAL DISEASE.

II.

Schizophrenia.

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Knowledge of cholesterol metabolism in the human body is as yet quite rudimentary. There has, nevertheless, accumulated a large body of literature concerning this substance. In spite of this large literature there is but casual mention of any possible significance which this sterol may play in neural function, and even rarer mention of any possible relationship which this substance may bear to mental disorder. Inasmuch as schizophrenia presents a problem of appalling dimensions and gravity, it is of utmost importance that all angles of it be fully investigated. Cholesterol represents one of these angles, and a greatly neglected one. It is the purpose of this paper to investigate the relationship which this substance may bear to schizophrenia.

Survey of Literature.

Pighini,[1] in 1910, seems to be the first to have attempted a correlation between blood cholesterol and schizophrenia. He found increased values for cholesterol in two cases of dementia præcox, and generally low values in chronic dementia præcox. In 1913 Parhon, Urechia and Popea[2] recorded increased cholesterol values in dementia præcox. DeCrinis[3] found increased values in the majority of his cases. Targowla, Badonnel and Berman[4] obtained normal values in two of their cases of this mental disease. Parhon and Parhon[5] found low values in chronic cases of dementia præcox but in general found the values to be inconstant. Gibbs[6] in 40 cases

*Work done while Commonwealth Fund Fellow in Psychiatry, Colorado Psychopathic Hospital, Denver, Colo.

of schizophrenia reported an average of 118.3 and 144.6 mg. respectively in 16 deteriorated males and in 11 deteriorated females. In five males in the acute stages he obtained an average of 104.6 mg. and in six females without deterioration, an average of 176.6 mg. Ornstein [7] in 40 cases of dementia præcox found 42.5 per cent with low blood cholesterol values, 35 per cent with increased values and 22.5 per cent with normal values. Forsyth [8] presented 22 cases of dementia præcox with blood cholesterol values ranging from 100 to 157 mg., which he states are *low* figures. He noted, also, cases with values of 167, 206 and 211 mg., the diagnosis in each of which he doubts. Goebel [9] in nine cases of dementia præcox found the blood cholesterol increased. Shaw and Sharpe [12] felt that the blood cholesterol content in dementia præcox is greatly diminished. They reported a series of 10 cases with a range of 30 to 85 mg. Duncan [10] found that schizophrenic states of confusion, excitement, agitation or depression lower the blood cholesterol more than schizophrenic states of quietness or apathy. In the dull or quiet cases he found that the average cholesterol was 214 mg. and in states of confusion or excessive emotion the average cholesterol was 179 mg. He used as his normal range, 140 to 200 mg. Stenberg [11] found that in dementia præcox the cholesterol values were raised and that they were normal during the remissions. He noticed also that emotionally dulled cases had a low blood cholesterol and emotionally heightened cases a high one.

Experimental Procedure.

Upon admission to the Colorado Psychopathic Hospital wards or out-patient department, patients received venipunctures from which were drawn under sterile conditions 5 cc. of blood. This blood was placed in a sterile oxalate tube (oxalate equivalent to 2 cc. 1 per cent solution) which was immediately stoppered, up-ended several times, and placed in an ice box until needed. Tubes which contained any clotted blood were discarded. The blood for the controls was obtained from willing individuals, not patients, who were judged normal from a clinical neuropsychiatric point of view.

The method used was a modified Myers-Wardell.[95] The blood in the oxalate tube was thoroughly mixed and 1 cc. of whole blood was transferred to a glass extraction thimble in which had been loosely

packed fat-free filter paper. This extraction thimble was placed in an oven in which the temperature varied between 70 degrees and 80 degrees Centigrade and dried for three hours. The extraction thimble was then placed in a 250 cc. Erlenmeyer flask with 20 cc. of C. P. chloroform. This flask was then attached to a water cooled reflux condenser and the blood extracted at 70 to 80 degrees Centigrade for two and a half hours. The chloroform and the material which it had removed from the blood were then transferred to a 25 cc. volumetric flask and made up to volume with chloroform at 20 degrees Centigrade. This flask was then placed in a water bath of 20 degrees Centigrade. Into each of two clean test tubes was placed 0.1 cc. of C. P. concentrated sulphuric acid and 2.0 cc. of C. P. acetic anhydride. To one tube was added 5 cc. of the "unknown" which had been made up to 25 cc. To the other was added 5 cc. of a standard solution of cholesterol in chloroform (5 cc. of standard cholesterol was equivalent to 0.4 mg. of cholesterol). To each tube was then added chloroform to a total volume of 10 cc. The tubes were again placed in a water bath at 20 degrees Centigrade and enclosed in a dark cabinet for fifteen minutes. The tubes are then matched—the unknown against the standard by means of a Dubosque colorimeter—and the number of mg. of cholesterol in 100 cc. of whole blood found by the calculation:

$$\frac{\text{Reading of Standard} \times 200}{\text{Reading of Unknown}} = \text{Number of mg. cholesterol in 100 cc. of whole blood.}$$

Duplicate cholesterol estimations are run upon each specimen of blood. In no instance was it found that the number of mg. per 100 cc. of cholesterol run on the duplicate varied more that 5 mg. This variation was considered to be within experimental error of the method. The average of the two cholesterol findings was presented in each case.

Discussion.

Cholesterol and Metabolism.

That cholesterol should be of any importance is interesting. Yet, exclusive of its water content, the brain is composed of about one-half lipoids,[14, 15] of which over 50 per cent are phospholipins and 25 per cent cholesterol. That it should actually play an impor-

tant part in cellular metabolism, especially in the cellular metabolism of the brain, is probably of even more interest. Nevertheless this is true, for there is an efficient regulation of cholesterol in the body, a regulation which is analogous to that of nitrogen metabolism.[13] This regulation is quite necessary inasmuch as an adequate lipoid metabolism seems to be particularly essential to the functional activity of the brain.[6] There is existent at the present time much evidence that the functional metabolism of the brain is characterized by a high degree of oxidation.[14, 16] Since phospholipins have an extreme susceptibility to oxidation,[17] it seems evident that the brain lipoids should have an active metabolic function. Certain outstanding facts regarding neural metabolism, therefore appear: first, the large amount of oxygen consumed by the brain of a waking animal—360 mg. per gm. per minute—as compared with that of the salivary glands which consume 28 mg., and of skeletal muscles which consume 4 mg.; second, the large percentage of phospholipins present, substances intimately bound up with the activation of vital processes; and, third, the presence of cholesterol in great excess as compared with other tissues. Cholesterol, obviously, should be of sufficient importance in neural metabolism to warrant careful study.

Moreover, there is an increasing amount of evidence that cholesterol has an essential part in lipoid metabolism in general and that this function is manifest in the relation of cholesterol to other blood lipoids, lecithin, fatty acids and neutral fat.[15, 19]

It is now generally recognized that under various physiological and pathological conditions the blood cholesterol may be lowered. Inasmuch as these conditions may be of importance, it may be wise to review them completely.

CONDITIONS WHICH LOWER BLOOD CHOLESTEROL.

In young actively growing infants it has been shown by Banu, Negresco and Heresco [20] and Norman [21] that the cholesterol values are lower than in the adult. In infants it has also been found that the blood cholesterol is lower in those artifically fed than in those that are breast fed.[22, 23, 24] Strathmann-Herweg,[25] however, could not demonstrate any difference in the blood cholesterol of infants during a six week period on breast milk or on artifical milk. Dolencourt

and Seisoff [22] found the blood cholesterol lowered in children with rickets and cranio-tabes, while Lesne, Sylvestre and Zizine [23, 24] found only eight cases of rickets out of 27 with blood cholesterol lower than normal although in five cases of cranio-tabes every one possessed a low blood cholesterol value. Strathmann-Herweg,[25] though, obtained normal values in ricketic children and in undernourished children with exudative diathesis. Attempts to decrease cholesterol in the blood by cholesterol-poor diets in human beings have been successfully carried out by Luden.[26] In this respect it is of interest that Langen [27] has pointed out that the human occupants of the Dutch Indies have blood cholesterol values far below the normal values of individuals living in countries where animal products constitute a great part of the diet. In cases of prolonged undernourishment Rosenthal and Patrzek [28] found lowered blood cholesterol values. Malerba [29] and Orlowski [30] also state that undernutrition may be the cause of low blood cholesterol. Strauss and Schubardt.[31] report that the inanition interferes with absorption from the intestines and that this lack of absorption may cause a lowered blood cholesterol. Rothschild and Wilensky [32] consider a diet which is poor in lipoids as a factor in producing hypocholesteremia. Hueck,[33] however, feels that it is probable that the body to a certain extent is independent of the amount of cholesterol in the food. Beumer and Bürger [34] and Cantieri [35] feel that the hypocholesteremia is dependent upon the nutrition and strength of the patient, while McCrudden and Sargent [36] assert that this state is arrived at due to low vitality.

Peterson and Levinson [37] found the blood cholesterol to be lowered in thin persons, while Arnoldi and Collazo [38] and Luden [39] found low values in obese people. The latter observation was not confirmed by Epstein and Lande [40] who examined one case, nor by Köning [41] who examined obese people suffering from disease and found an increased blood cholesterol in all of them.

Gibson and Howard [42] state that they find that the blood cholesterol is lowered in severe pernicious anemia. Cholesterol determinations in the blood plasma or serum of cases of pernicious anemia have also been recorded by Barat,[43] Klemperer,[44] Antonelli,[45] Strauss and Schubardt,[31] Myers,[46] MacAdam and Shiskin,[47] Beumer and Bürger,[34] Medak,[49] Barmvater,[50] Csonka,[51] Bloor and Knudson,[52]

Benda,[53] Denis,[54] Pacini,[55] Gorham and Myers,[56] Feigl,[57] deWesselow and Bamforth,[58] Schnabel,[59] Kahn and Barsky,[60] Stern,[61] King,[62] Kipp,[63] Rothschild and Felson,[64] Stepp,[65] Richter-Quittner,[66] Köhn[67] and Rosenthal and Holzer.[68] There were 172 cases in all and of these 47 cholesterol values were within normal limits, 18 were above normal and 107 were below normal limits. Muller[69] who also studied this group of cases found recorded red cell counts in 101 cases. Of these cases he found that 84 had a 50 per cent reduction of red blood cells. Of these cases 46, or 54.7 per cent, had cholesterol values lower than normal. He states that, in this group of 101 cases, when the red cell count in these individuals ranged from zero to three million the cholesterol values in the blood were 6.6 per cent above normal, 30.1 per cent normal and 42.9 per cent below normal, and that " with a stationary cholesterol level, the red blood cells may remain stationary or show increase or decrease, while if the cholesterol increases the red blood cells are apt to increase. A lowering of the cholesterol level may be accompanied by a rising blood count." This lack of correlation in the relation of the number of red blood cells and also of hemoglobin values and the blood cholesterol has been noticed by MacAdam and Shiskin,[47] Köhn,[67] Beumer and Bürger[34] and Denis.[54] Bloor and MacPherson,[46] Schnabel[59] and Strathmann-Herweg[25] feel, however, that the number of red cells is directly related to the low values of cholesterol. Muller,[70] in a study of a series of pernicious anemia patients, found that " the cholesterol in the blood plasma was low in relapse but that a sudden rise to a higher level, which later increases and was maintained, occurred at the onset of the remission."

Wendt[71] states that the cholesterol esters are commonly lowered in individuals with severe icterus. Muller[69] finds that " it is striking how frequently hypocholesteremia is encountered in hemolytic jaundice in spite of the fact that, comparatively speaking, the anemia is not severe." Neilson and Wheelon,[72] Obakewicz[73] and Oulmont and Boidin[74] feel that in familial hemolytic jaundice the blood cholesterol is decreased. Stern,[75] Gibson and Howard,[43] Antonelli,[45] Csonka,[51] Barmvater,[50] Eppinger and Ranzi,[76] Barat,[43] Feigl,[57] Chauffard, Laroche and Grigaut,[77] Rosenthal and Holzer,[68] King,[62] MacAdam and Shiskin,[47] Parisot and Heully,[78] and Medak[49] report blood cholesterol determinations on hemolytic jaundice. Thirty-seven cases were listed. These cases were studied by Muller[69] who found that " 25

or 67.5 per cent showed a hypocholestermia. Of the remaining 12, two showed hypercholesteremia while the other 10 were within normal limits." Muller [70] reports a case in which cholesterol values in the plasma were below 50 per cent of the normal. The blood cholesterol values observed in hemolytic jaundice are of much interest because the patients with this disease are also in relatively good health and nutrition. Berman [79] describes a syndrome of anorexia, muscular weakness and marked fatigability in which there is a lowered blood cholesterol. He remarks, also, that the blood cholesterol is lowered in pernicious anemia, Addison's disease, exophthalmic goiter and toxic hyperthyroidism. Epstein and Lande,[40] Mason, Hunt and Hurxthal,[80] Bing and Heckscher,[81] and Heckscher [82] feel that the blood cholesterol is decreased in hyperthyroid conditions. Leupold and Seisser [83] showed that the feeding of thyroid or the injection of thyroid extract produced a reduction of the cholesterol in blood while large doses of thyroid produced just the opposite effect. Goldzieher and Hirschorn [84] report that thyroid administration cause an increased storage of cholesterol in the Kupffer cells of the liver. Gainsborough [85] states that a lowered blood cholesterol may occur at the height of a fever and in cases of enlarged prostate with infection of the urinary tract. Kipp [86] has called attention to the existence of hypocholesteremia in acute infections such as pneumonia, and he has pointed out that the variation of blood cholesterol depends on the activity of the leukocytes inasmuch as cholesterol was transported by them to the area of infection. Gibbs [6] feels that the blood cholesterol is low in severe anemias and acute infections. Muller [69] in his analysis of secondary anemias finds no justification for the conclusion that a hypocholesteremia exists in them. Low blood cholesterol values have been reported in advanced pulmonary tuberculosis.[87]

Calvin and Goldberg [87] state that decreased blood cholesterol may be due to increased secretion of bile, to increased deposits of cholesterol in the liver or other organs, or to an increased activity of the reticulo-endothelial system. Leites [88] in a study of the relation of the reticulo-endothelial system in dogs found that the blood cholesterol was decreased temporarily from 30 to 50 per cent after the injection of colloidal silver. The same results were obtained by Goebel and Gnoinsky [89] in dogs and Beumer [90] in rabbits. Muller [69] feels that

in hemolytic anemias the decrease in the cholesterol is out of proportion to the decrease of the erythrocytes, probably owing to the increased activity of the hematopoietic system. Berberick [91] suggests that low blood cholesterol may be due to a constitutional weakness of the endocrine system. Abelous and Soula [92] showed that in autolysis of the spleen there was a decrease of the cholesterol content, while Marino [93] showed that the same was obtained in autolysis of the thyroid, adrenals, spleen and kidneys. Rosenthal and Holzer [94] state that low cholesterol values are found occasionally in retention icterus. Jarno [75] has reported that in clinical conditions with hyperacidity, a low blood cholesterol may be found.

It is felt that in order to justify a statement that some cases of schizophrenia possess a low blood cholesterol, it is necessary to consider in the cases presented the presence or absence of these factors which in themselves may alter the blood cholesterol. When this is done, the relationship of cholesterol to schizophrenia may be considered, and not before.

DISCUSSION OF RESULTS OBTAINED.

In the present study there are 55 cases of schizophrenia with a range of blood cholesterol values of 55 to 300 mg., (Table 1), in which range there is a definite concentration of cases in the lower half and a corresponding paucity of cases in the upper half. There are no cases between 162 and 272 mg., or between 272 and 300 mg. This type of distribution is so unbalanced that it must be definitely considered in evaluating the results. The range of cholesterol values in normal individuals is 110 to 195 mg. (Table 1).

TABLE 1.

THE DETAILED DISTRIBUTION OF CHOLESTEROL VALUES AS MEASURED IN MG. PER 100 CC. OF WHOLE BLOOD IN BOTH NORMAL AND SCHIZOPHRENIC GROUPS.

No. of cases.	Normal values.	No. of cases.	Schizophrenic values.	No. of cases.	Normal values.	No. of cases.	Schizophrenic values.
..	...	1	55	..	...	4	133
..	...	1	60	2	136	1	136
..	...	1	64	1	139	..	...
..	...	1	69	3	140	..	...
..	...	2	72	2	142	2	142
..	...	1	78	1	146	..	...
..	...	3	80	1	148	..	...
..	...	2	83	1	149	..	...
..	...	1	85	3	150	..	...
..	...	1	86	1	151	..	...
..	...	1	88	1	153	1	153
..	...	3	90	3	155	..	...
..	...	1	92	1	157	1	157
..	...	1	93	1	158	..	...
..	...	1	95	1	159	..	...
..	...	1	96	3	160	1	160
..	...	2	98	2	162	1	162
..	...	4	100	1	163	..	...
..	...	1	102	2	165	..	...
..	...	3	103	1	167	..	...
..	...	1	106	1	169	..	...
1	110	1	110	1	170	..	...
..	...	1	111	1	172	..	...
..	...	1	112	1	173	..	...
1	113	..	...	2	177	..	...
..	...	1	115	2	180	..	...
1	118	..	...	1	183	..	...
1	122	..	...	1	186	..	...
1	125	..	...	1	188	..	...
1	126	..	...	1	195	..	...
1	128	..	...	..	...	1	272
2	130	5	130	..	...	1	300
1	132	..	...				

TABLE 2.

THE DETAILED DISTRIBUTION OF CHOLESTEROL VALUES AS MEASURED IN MG. PER 100 CC. OF WHOLE BLOOD IN THE VARIOUS TYPES OF SCHIZOPHRENIA.

MG. CHOLESTEROL IN INDIVIDUAL CASES OF SCHIZOPHRENIA.

Simple.			Hebephrenic.			Catatonic.			Paranoid.		
	(60)			(64)						(55)	
	(78)			(72)						(69)	
	(85)			(72)						(80)	
	(86)	55.5% below normal		(80)	85.7% below normal			27.3% below normal		(80)	
	(90)						(90)			(83)	72.2% below normal
	(96)			(95)			(93)			(88)	
	(98)									(90)	
	(98)						(100)			(92)	
94.3%	(100)		100%	(103)		72.7%			88.8%	(100)	
below	(103)		below			below			below	(100)	
normal	(111		normal	(112		normal	(110		normal	(102)	
mean	(130		mean			mean	(115		mean	(106)	
	(130						(130				
	(130									(130	
	(133	38.8% within normal limits			14.3% within normal limits		(133	63.6% within normal limits		(133	27.8% within normal limits
	(133						(142			(136	
	(142						157			153	
							160			162	
	272	5.5% above normal					300	9.1% above normal			
Median:		101.5	Median:		87.5	Median:		130	Median:		96

NOTE.—Percentage on basis of individual groups as entities.

TABLE 3.

SHOWING THE CONSTANTS DERIVED FROM THE STUDY OF THE NORMAL AND OF THE SCHIZOPHRENIC GROUPS.

Constants.	Normal.	Schizophrenic group.
Range	110–195	55–300
Median	150	100
Mean	148 ± 1.7	Not computed *
Cases below normal	...	33
Cases within normal	54	20
Cases above normal	...	2
Total number of cases	54	55
Percentage of cases below normal	...	59.99
Percentage of cases within normal	100	36.36
Percentage of cases above normal	...	3.63
Cases below normal mean	20	49
Cases above normal mean	34	6
Percentage of cases below normal mean	37	89.08
Percentage of cases above normal mean	62.9	10.90

* Due to the fact that there is an unbalanced distribution of blood cholesterol values.

TABLE 4.

CORRELATION OF BLOOD CHOLESTEROL AS MEASURED IN MG. PER 100 CC. OF BLOOD AND CLINICALLY DETERMINED BODY TYPE.
FIVE CASES WERE NOT AVAILABLE.

BODY TYPE.								
Pyknic.			Athletic.			Asthenic.		
			60			55		
	72		64			69		
	78		83			80		
	86		83			80		
	88		85			80		
	90	52.9%		56.3%		90	79.3%	
		below		below		90	below	
	92	normal	95	normal		93	normal	
			96			98		
						98		
	100					100		
88.2%	100		81.2%			100%	100	
below	103		below	102		below	103	
normal			normal	103		normal	106	
mean	130		mean	110		mean		
	130			111				
	130			130				
	130	47.0%			31.2%		133	17.6%
	133	within			within		133	within
	136	normal		142	normal		142	normal
	153	limits			limits			limits
	157			162				
				272	12.5%			
				300	above			
					normal			
Median:	103		Median:	102.5		Median:	98	

NOTE.—Percentage on basis of individual groups as entities.

In correlating the clinically observed body type, of which 50 were available, and blood cholesterol (Table 4) no definite relationship is noted. The distribution of the blood cholesterol values among the three body types is relatively even. This is brought out more clearly by the three medians: pyknic, 103 mg.; athletic, 102.5; and asthenic, 98 mg. This is also demonstrated by a study of the percentage distribution of the blood cholesterol in the individual body type groups (Table 4). There are definitely more asthenic patients with blood cholesterol values below the lower limits of normal and also below the normal mean than in either the pyknic or athletic types, whereas the reverse is true of the cholesterol values within normal limits. There were two cases in which the blood cholesterol values were above normal limits. Both of these cases were of the athletic body type.

Table 5 is a correlation between the state of abnormal nourishment and blood cholesterol. Of the 55 cases, 3 were not available,

TABLE 5.

CORRELATION OF STATE OF ABNORMAL NOURISHMENT OF PATIENT AND BLOOD CHOLESTEROL.

Obese.		Undernourished.
		55
72		72
	1.8%	90 10.8%
	below	93 below
	normal	100 normal
		100
	3.6%	115 5.4%
130	within	133 within
157	normal	133 normal
	limits	limits

NOTE.—Percentage on basis of fifty-five cases.

40 were normal, and 12 were abnormally nourished. Of these 12, 3 were obese and 9 were undernourished. A study of these 12 cases shows no correlation between the state of abnormal nourishment and blood cholesterol. All of the undernourished individuals were of the asthenic habitus.

Chart 4 is a scattergram of the red cell count of the schizophrenes and the corresponding blood cholesterol. The red cell count falls between the extremes of 3,600,000 and 5,500,000, a range that is

relatively normal. Throughout the entire range of red cell values the cholesterol values show no tendency toward unbalanced grouping. No relationship is found between the observed red cell count and blood cholesterol. The correlation of the percentage of hemoglobin and blood cholesterol is presented in Chart 5. The range of hemoglobin is 60 to 96 per cent and the blood cholesterol values show no trend towards unbalanced grouping. No relationship could be established. Chart 6 is a correlation between the white cell count and blood cholesterol. The white cell count shows a range of 4,500 to 19,500. Here, too, there appeared no unbalanced grouping of cholesterol values, and no correlation could be established. Charts 1, 2 and 3 are scattergrams of the systolic, diastolic and pulse pressures, respectively, and of blood cholesterol. Again the correlations in each of these charts show no unbalanced grouping of cholesterol values and demonstrate no significant relationships in any of the three correlations. Table 6 is a correlation of cholesterol and ab-

TABLE 6.

CORRELATION OF ABNORMAL CHEMICAL PRODUCTS IN THE URINE WITH BLOOD CHOLESTEROL.

URINE.					
Indican.		Albumin.		Acetone.	
55					
72					
78	12.7%		3.6%		5.4%
83	below		below		below
83	normal		normal	98	normal
96		90		98	
100		100		103	
	5.4%	130	9.0%	110	3.6%
133	within	133	within		within
157	normal	133	normal		normal
160	limits	133	limits	160	limits
		300			

NOTE.—Percentage on basis of fifty-five cases.

normal chemical products in the urine. In 20 cases there was indican ten times, albumin seven times. and acetone five times. The range of cholesterol in each type of abnormal chemical product is such that no relationship between these products and cholesterol was found.

Chart I

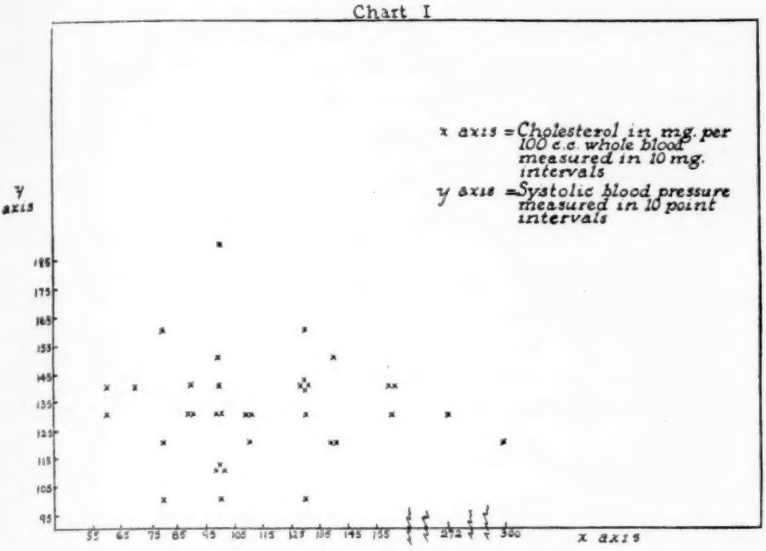

x axis = Cholesterol in mg. per 100 c.c. whole blood measured in 10 mg. intervals
y axis = Systolic blood pressure measured in 10 point intervals
y axis
x axis

Chart III

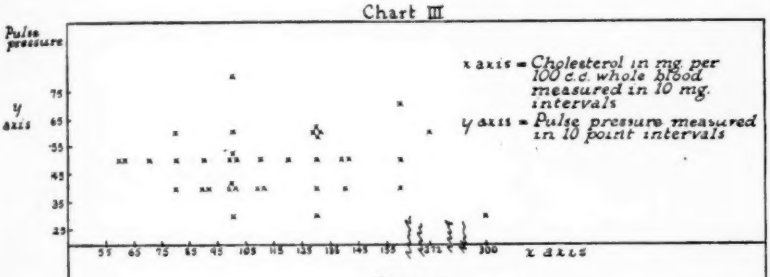

Pulse pressure
y axis
x axis = Cholesterol in mg per 100 c.c. whole blood measured in 10 mg. intervals
y axis = Pulse pressure measured in 10 point intervals
x axis

Chart II

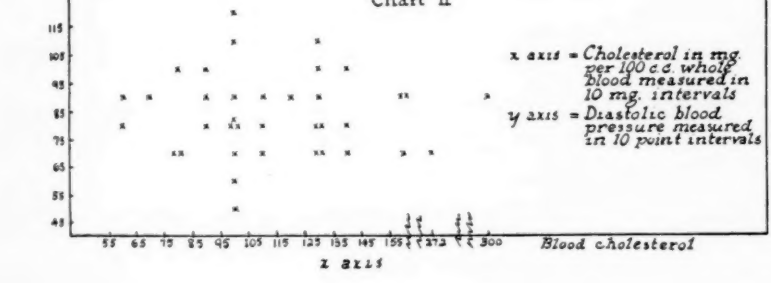

x axis = Cholesterol in mg. per 100 c.c. whole blood measured in 10 mg. intervals
y axis = Diastolic blood pressure measured in 10 point intervals
Blood cholesterol
x axis

Chart V

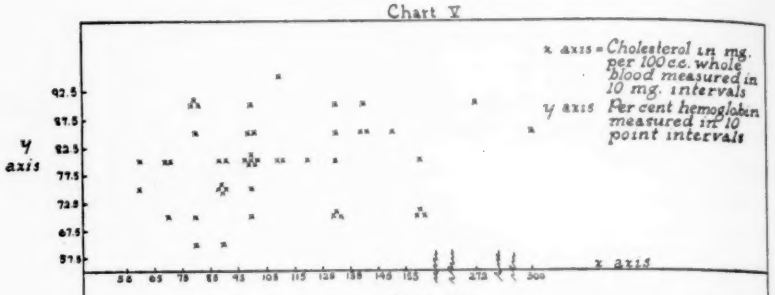

x axis = Cholesterol in mg. per 100 c.c. whole blood measured in 10 mg. intervals
y axis Per cent hemoglobin measured in 10 point intervals
y axis
92.5
87.5
82.5
77.5
72.5
67.5
57.5
55 65 75 85 95 105 115 125 135 145 155 272 300
x axis

Chart IV

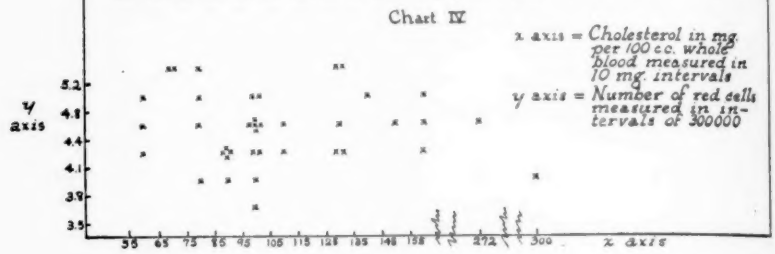
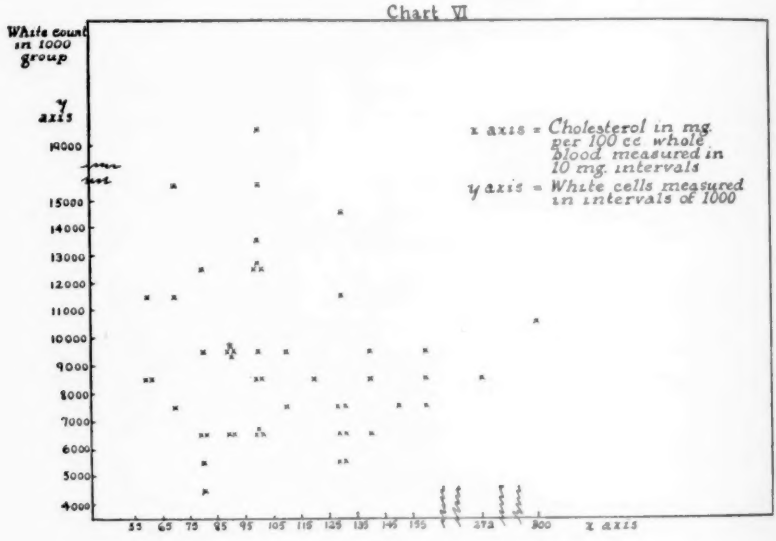

x axis = Cholesterol in mg per 100 c.c. whole blood measured in 10 mg. intervals
y axis = Number of red cells measured in intervals of 500000
y axis
5.2
4.8
4.4
4.1
3.8
3.5
55 65 75 85 95 105 115 125 135 145 155 272 300
x axis

Chart VI



Thirty-four cases presented physical abnormalities of 22 types, (Table 7). The number of cases in each type of physical abnormality is too small to draw any definite conclusion. Table 8 is a correlation between blood cholesterol and the state of mental and physical activity, separately and in their various combinations. A study of the table reveals no probable relationship existing in the

TABLE 7.

A Correlation of Blood Cholesterol and Observed Physical Abnormalities.

Acne-like lesions	60	80	86	90	93	...	130	...	...	...
Post-nasal discharge	..	..	..	92	100	102	133	133	142	...
Dental caries	..	..	..	92	100	103	130	...	...	...
Systolic murmur	..	83	83	88	...	103	...	...	...	...
Slight cervical adenopathy.	60	80	..	..	...	102	130	...	...	...
Questionable pulmonary tuberculosis	..	..	..	..	...	103	130	...	...	272
Leucorrhea	..	72	90	93	...	...	...	...	...	...
Slight inguinal adenopathy	..	..	..	..	...	102	...	...	142	162
Hypothyroid	..	..	85	..	...	...	...	...	...	300
Inflamed pharynx	..	..	..	..	...	102	...	...	142	...
Palpable liver	..	78	..	..	...	103	...	...	...	...
Arteriosclerosis	..	..	..	..	...	...	130	...	...	...
Bronchitis	..	..	..	96	...	...	...	...	...	...
Inflamed tonsils	..	80	..	..	...	...	...	...	...	...
Questionable gastric carcinoma	..	..	..	92	...	...	...	...	...	...
Discharging ear	..	..	..	90	...	...	...	...	...	...
Pregnancy	..	..	..	..	...	103	...	...	...	...
Hernia	..	..	..	..	...	110	...	...	...	...
Deafness	..	..	..	..	...	102	...	...	...	...
Prostatitis	..	..	..	..	...	103	...	...	...	...
Myocarditis	..	..	..	..	...	103	...	...	...	...
Pyorrhea	..	..	..	..	...	...	130	...	...	...

various groups and the blood cholesterol as is brought out more clearly by comparing the medians and the percentage groupings.

Having considered all of the types of pathology that might lower the blood cholesterol values of the schizophrenic group and being unable to substantiate their existence as definitely influencing these values in this study, although some of the correlations are unquestionably interesting, it is possible to proceed with the consideration of the cholesterol values in schizophrenia as an entity.

TABLE 8.

CORRELATION OF BLOOD CHOLESTEROL AS MEASURED IN MG. PER 100 CC. OF WHOLE BLOOD AND TYPE OF ACTIVITY. INCREASED PHYSICAL—RESTLESS, FIGHTING, PACING, UNABLE TO ATTAIN "NORMAL" PHYSICAL COMPOSURE, ETC. DECREASED PHYSICAL—QUIET, SITTING, INDISPOSED, INACTIVE, ETC. INCREASED MENTAL—ACTIVE MENTAL STATE, INCREASED PRESSURE AND FREEDOM OF THOUGHT, ACTIVE DELUSIONS, ILLUSIONS, HALLUCINATIONS, ETC. DECREASED MENTAL—INACTIVE MENTAL STATE, QUIET, POVERTY OF THOUGHT, DEPRESSED, APATHETIC, ETC. NORMAL—ACCEPTED TERMINOLOGY.

	Physical.			Mental.			Physical and mental.					Physical normal.		Mental normal.	
Mg. cholesterol.	Increased.	Normal.	Decreased.	Increased.	Normal.	Decreased.	Increased.	Normal.	Decreased.	Physical increased. Mental decreased.	Physical decreased. Mental increased.	Mental increased.	Mental decreased.	Physical increased.	Physical decreased.
55	...	1	...	...	...	1	...	...	...	...	...	...	1	...	...
60	...	...	1	...	1	...	...	...	...	...	...	...	...	...	1
64	1	...	...	1	...	...	1	...	...	...	...	...	...	...	...
69	...	...	1	...	1	...	...	...	...	...	...	...	...	...	1
72	2	...	...	2	...	...	2	...	...	...	...	...	...	...	...
78	...	1	...	1	...	...	...	...	...	...	...	1	...	...	...
80	1	1	1	1	2	...	1	1	...	...	...	...	...	...	1
83	...	1	1	...	1	1	...	1	1	...	...	...	...	...	...
85	...	1	...	...	...	1	...	...	...	...	...	...	1	...	...
86	...	...	1	...	...	1	...	...	1	...	...	...	...	...	...
88	...	1	...	...	1	...	...	1	...	...	...	...	...	...	...
90	...	1	2	...	1	2	...	1	2	...	...	...	...	...	...
92	...	1	...	...	1	...	...	1	...	...	...	...	...	...	...
93	...	...	1	...	...	1	...	...	1	...	...	...	...	...	...
95	1	...	...	1	...	...	1	...	...	...	...	...	...	...	...

96	...	1	...	...	1	...	...	1	...	...	...	...	...	...	...	
98	2	...	...	2	...	...	2	...	...	...	...	...	...	...	...	
100	2	...	2	3	...	1	2	...	1	...	1	...	...	...	...	
102	...	...	1	...	...	1	...	...	1	...	...	...	...	...	...	
103	1	...	2	1	1	1	1	...	1	...	...	...	...	...	1	
106	...	...	1	...	...	1	...	...	1	...	...	...	...	...	...	
110	...	...	1	...	...	1	...	...	1	...	...	...	...	...	...	
111	...	1	...	1	...	...	...	...	...	...	...	1	...	...	...	
112	...	1	...	...	1	...	...	1	...	...	...	...	...	...	...	
115	...	...	1	...	...	1	...	...	1	...	...	...	...	...	...	
130	1	1	3	1	1	3	1	1	1	...	...	...	1	...	1	
133	1	1	2	2	2	...	1	...	...	...	...	1	...	...	2	
136	...	1	...	1	...	...	...	...	...	...	...	1	...	...	...	
142	1	1	...	1	1	...	1	1	...	...	...	...	...	...	...	
153	...	1	...	1	...	...	...	...	...	...	...	1	...	...	...	
157	...	1	...	...	1	...	...	1	...	...	...	...	...	...	...	
160	...	...	1	...	1	...	...	...	...	...	...	...	...	...	1	
162	...	1	...	...	1	...	...	1	...	...	...	...	...	...	...	
272	...	...	1	...	...	1	...	...	1	...	...	...	...	...	...	
300	1	...	...	1	...	...	1	...	...	...	...	...	...	...	...	
Median	100	111	103	100	103	102	100	96	103		Insufficient cases					
Per cent below normal	18.1	14.6	25.4	21.8	18.1	20	18.1	10.8	16.3	0	1.8	1.8	3.6	0	9.3	
Per cent within normal	5.4	16.3	14.6	12.7	14.6	9	5.4	7.3	9.3	0	0	9.3	1.8	0	9.3	
Per cent above normal	1.8	0	1.8	1.8	0	1.8	1.8	0	1.8	0	0	0	0	0	0	
Per cent below normal mean	23.6	25.4	38.2	32.7	27.3	29	23.6	14.6	23.6	0	1.8	9.3	5.4	0	12.7	

Table 2 shows the relationship of the cholesterol values in the four types of schizophrenia. A survey of the groups shows the values to be low in each of them and to be relatively higher in the catatonic group than in the other three, whose grouped values appear to be somewhat similar with a greater concentration of low values in the hebephrenic group. This is brought out much more clearly in a study of the medians: simple, 101.5; hebephrenic, 87.5; catatonic, 130; and paranoid, 96 mg. This fact is also revealed by the percentage grouping.

Table 3 is a study of the relationship of the cholesterol values in normal and schizophrenic individuals. It is obvious that, in general, the schizophrenic group has a range which begins much lower than the normal and extends into much higher values (two cases). The mean of the normal group is 148± 1.7 mg., which is approximately that of the median, 150 mg. The median of the schizophrenic group is 100 mg., a value exceedingly lower than that obtained for the normal group and even below the lowest limits of the normal group. Of the schizophrenic group there are 33 cases (59.9 per cent) below the normal range, 20 cases (36.3 per cent) within the normal range, and 2 cases (3.6 per cent) above the normal range of cholesterol values. Moreover, of the schizophrenic group, 49 cases (89.1 per cent) fall below the position of the normal mean of cholesterol values whereas only 6 cases (10.9 per cent) fell above it. From a study of these figures, it is obvious that there is a definite alteration of the blood cholesterol values in schizophrenia as compared to the normal values. These cholesterol values in the schizophrenic group as a whole are definitely below normal and in the range below the normal mean. To speculate as to the cause of this condition would be interesting but it is not the purpose of this paper to do so.

SUMMARY.

1. A study of the blood cholesterol in schizophrenia is presented.

2. Many of the factors which might alter the blood cholesterol other than that induced by the mental disorder itself are investigated.

3. There is a definite tendency to a lowered blood cholesterol in schizophrenia as a group.

4. There are cases of schizophrenia in which the blood cholesterol is normal or increased, but the majority of cases show values below the lower limit of normal or in the lower limits of the normal range.

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SCHIZOPHRENIA IN CHILDREN.*

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If the central nervous system is the functional medium by which thinking, feeling and acting are expressed, then it is to be expected that deviations of thinking, feeling and acting are apt to be different in children than in the adult because the central nervous system, especially in respect to its more complex differentiations, is in a developmental stage. Consequently, the central nervous system in the child is unable to respond with the complexity of the adult in its functional expression.

In discussing schizophrenia in children, it is important to give consideration to the facilities which children possess for expressing any form of psychopathology. It is necessary to bear in mind that children cannot be expected to exhibit psychopathology with all the elaborations of the adult. It must be remembered that the level of intellectual development and the life experiences of the child are limited in comparison with those of the adult.

Language developed to a degree of complexity is a product of a mature intelligence. Children, therefore, do not possess the facility to fully verbalize their feelings, nor are they capable of complicated abstractions. Consequently, delusional formations seen in childhood are relatively simple and their symbolization is particularly naive. There is a law [1] formulated by Storch and elaborated by Domarus [2] that, "Thought and language in their development change from feeling, concreteness and perception in the direction of reasoning, differentiation and abstraction."

A comparison may be made of the psychopathological expressions of children and primitive races. Among the primitive races, mental disease, especially dementia præcox, is uniformly simple in its clinical expression. Kraepelin [3] found among the Javanese a preliminary period of confused excitement which rapidly passed

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into simple dementia. He found that hallucinations were rare and that the delusional formation was elementary and of uncomplicated construction.

Children are essentially beings of feeling and behavior. Consequently, their psychopathology may be expected to be expressed largely through distorted affective responses and altered behavior reactions.

The term "children" should include only such persons who show no physical evidences of puberty and are largely in the first decade of life. For purposes of clarity, the term "schizophrenia" in this communication is limited to such reaction types as are characterized by:[4, 5, 6, 7, 8]

1. A generalized retraction of interests from the environment.
2. Dereistic thinking, feeling and acting.
3. Disturbances of thought, manifested through blocking, symbolization, condensation, perseveration, incoherence and diminution, sometimes to the extent of mutism.
4. Defect in emotional rapport.
5. Diminution, rigidity and distortion of affect.
6. Alterations of behavior with either an increase of motility, leading to incessant activity, or a diminution of motility, leading to complete immobility or bizarre behavior with a tendency to perseveration or stereotypy.

Psychodynamically, regression is a characteristic of schizophrenia in children as well as in adults.

In a review of the literature, several reports were available which dealt largely with psychoses or insanity in children, rather than with specific reaction types. In some of the reports, the various psychotic reaction types were specified; but in others, it was rather difficult to determine, except from a careful analysis of the individual case reports, what specific reaction types were under discussion. It was also found that the term "children" was used rather loosely and included patients with ages as high as 16 or 17 years. Weichbrodt[9] reports two cases of dementia præcox appearing in ten year old children. One was already deteriorated at the time of examination. He called attention to the evanescent quality of their delusional thinking. Rümke[10] reported 16 cases of psychoses occurring in children, two of whom were nine years of age—two, 10 years—one, 11 years—two, 12 years—four, 13 years—one, 14

years—two, 16 years and two, 17 years. None of these cases was classified in the dementia præcox or schizophrenic group. He called attention to the fact that the psychoses in these children were expressed in a very primitive form and that in most a Freudian mechanism was unquestionably in the foreground. Rhein,[11] in 1920, reviewed 44 cases from the literature, the majority of whom were suffering from psychopathies of a definitely encephalopathic type. One of his cases, a 14 year old child, was diagnosed as a case of dementia præcox. Hart [13] reports a case in detail—a boy seven years old in whom there was dereistic thinking, diminished affect, inactivity, general retraction of interests from the environment, distorted affective response and visual hallucinations. Although he did not offer schizophrenia as a diagnosis, it seemed to the writer that such a diagnosis was warranted. Plant,[12] in an article discussing the psychiatric value of certain psychological tests, refers to four cases of dementia præcox in children, but gives no description concerning them. Kassanin [14, 17] and Kaufman report that of a total of 6000 cases admitted to the Boston Psychopathic Hospital, there were 16 under the age of 16 years diagnosed as psychotic. These authors conclude that six of these belonged undoubtedly to the schizophrenic group, with ages ranging from 13 to 15 years. Brill [15] reports four cases of psychoses in children, two of whom, four and a half years and six years old respectively, were diagnosed as dementia præcox of the catatonic form. Strecker,[16] in a review of 5000 consecutive hospital admissions, found only 18 cases under 15 years of age who presented a psychosis. Four of these were cases of dementia præcox and their ages ranged between 11 and 14 years. The two 11-year-old cases were possibly not yet in adolescence, although no definite statement was made by the author concerning this.

The six cases reported in this communication represent typical schizophrenia in children. All of them were or are patients under observation and treatment in the Psychiatric Institute. Their actual ages on admission were 4, 6, 10, 11, 11 and 12 years respectively. The three cases who were over 10 years of age on admission presented undoubted evidences of the existence of their schizophrenia long before they were 10 years of age. With the exception of one case, none showed any physical indication of puberty. This case showed a few scanty hairs of the secondary type about the

symphysis but his psychosis had its initiation before he was five years of age.

CASE I (No. 598).—A Jewish boy who was admitted to the Psychiatric Institute at the age of four years because of laughing spells, abstraction, and mutism. He is an only child of parents who are apparently happily mated. The mother is the more aggressive parent, but a capable, intelligent young woman with a well-integrated personality. The father is a well-educated individual, rather quiet, much attached to the patient, but submissive to his wife's aggressiveness. The mother has always maintained an over-protective attitude toward the patient, feeding him, dressing him and never encouraging him to do things for himself up to the time of admission to the institute. The birth was uneventful, he was a well-nourished bottlefed baby, who teethed, walked and talked at the usual ages. From infancy he was always active, happy, bright, alert, talkative and good natured. When out in the park, always attended by his mother, grandmother or nursemaid, he depended largely upon the adult accompanying him. He seemed indifferent to other children and never attempted to join in play with them. He was afraid of dogs and cats. He had no serious illness. He showed what might be regarded as a normal affection toward both parents. His wishes were rarely thwarted.

About four months prior to admission, when he was three and a half years of age, he began to be irritable, cross, very destructive and over active in a purposeless way. He soon became unmindful of those about him, paid no attention to their questions or commands, became more quiet, spent most of his time in bed whispering to himself and playing with his fingers. A few days later he began to talk aloud again, but his talk was meaningless to his parents and his answers to questions totally irrelevant. He insisted on remaining in bed, and his face wore a fixed, blank, expression. At times he was exceedingly stubborn and negativistic, and on occasion he would bite, scratch and hit his mother in a rage. At times he would masturbate openly.

On admission to the institute, there was some purposeless over-activity at first. He would have rages in which he would tear his clothing. After a few days he began to be more passive in his behavior. There were periods of unprovoked crying. He was usually quite indifferent to the visits of his mother. Much of the time he would be found standing about the ward in a rigid motionless attitude. He showed a marked hyper-suggestibility and would maintain postures in which he was placed for indefinite periods. On rare occasions he would spontaneously make brief four or five word sentence comments with regard to something in the environment. Except for these occasional comments, he was mute. On occasions he would break out in an unprovoked laugh. His affect was an admixture of abstraction, indifference, self-absorption and to a certain degree, apprehension. His sensorium appeared clear throughout. Although a psychometric test was impracticable, it seemed obvious that he was a boy of at least average intelligence. During the three-month period under observation at the institute, there were occasional unpredictable outbursts of active resistance accompanied by rage, lasting for a period of several minutes each.

Discussion.—The symptomatology consisting of hyper-suggestibility, attitudinizing, mutism, abstraction, indifference with short periods of excitement, and a generalized retraction of interests from the environment warrant a diagnosis of schizophrenia of the catatonic form. A search for constitutional or organic factors in the situation discloses not even a suggestion. A dominant over protective mother with a submissive type of father, together with such a rearing in which all the patient's needs were supplied for him without effort on his part and lack of opportunity to integrate himself outside of the family group certainly is likely to foster an attitude of dependence, especially directed toward the mother.

CASE II (No. 620).—A Jewish boy, six and a half years of age on admission to the Psychiatric Institute, who had been dismissed from school because of senseless giggling, jumping up and down in his seat, wandering aimlessly about the class room, and grimacing. He is the younger of two children, the sister being five years his senior and of a bright, vivacious, friendly, sociable type. Both parents had a common school education. The father is a quiet, phlegmatic, unassertive type, while the mother is over solicitous, anxious and wants to be constantly reassured that she is not guilty of neglecting the patient by keeping him in the hospital. It is clear that the patient has served as an outlet for the mother's emotional needs. There are no obvious disharmonies in the family group. Other members of the direct and collateral ancestry show no outstanding psychopathological traits.

The patient's birth was uneventful. No feeding difficulties were experienced and he was a well-nourished infant. At the age of six weeks he developed an otitis media which soon involved the mastoid, necessitating mastoid incision and drainage, running a chronic course over a period of nearly two years and requiring frequent surgical dressings. The patient, being an infant, naturally has to be restrained at each sitting and because the mother was the active participant in his restraint, she thinks that he developed a hatred for her, manifested by his refusing to voluntarily come to her until nearly three years of age. There were no indications of a cerebral involvement during or following the mastoiditis. During the course of the mastoid trouble, he was very fretful and cried a great deal at night. He walked and talked at the usual ages.

As a pre-school child and since then he has had an over-protected existence. He seldom would be allowed out by himself, never had the usual opportunities to mix with other children except at school, and was escorted to and from school by his mother or older sister. When four years of age, *i. e.*, two and a half years prior to admission, he became reticent, responded irrelevantly or not at all to questions and blinked. He lost interest in things about him, became under active and seemed preoccupied. There was a gradual return to his usual quiet submissive dependent attitude within six

months of the onset of the condition. Shortly after the disappearance of these symptoms he entered kindergarten. At kindergarten he spent most of his time wandering aimlessly about the room, quietly giggling or chuckling to himself. His interest could never be held long enough to get him to finish a task. At times he was extremely talkative, always about his family and in a disconnected manner, so that it was difficult for the teacher to follow his ideas. There were occasional temper outbursts usually provoked by an attempt to interfere with his wandering about the room. He took no interest in the other children except to now and then impulsively grab their work and destroy it. The kindergarten teacher thought he was of average ability but unable to apply himself. After spending one year in kindergarten he entered the first grade. His teacher in the first grade noticed nothing unusual about him for the first three months. Then, about six months prior to admission, when he was six years old, he began to neglect his work and would sit staring into space. He seemed to the teacher to be forgetful. At times he would burst out crying and when questioned he said he was afraid his mother would not be home when he got there. During the summer vacation just prior to his admission it was noticed that he especially avoided other children in the street. He took no interest in his toys. He would spend much of his time picking up bits of paper and arranging them in rows. He became very unhappy because his sister was away on a visit. Later he became somewhat restless, flooded the bath room, meddled with the electric light switches, walked up and down the street indifferent to the surroundings, making stereotyped motions with his hand in the air as though writing, constantly blinking, repeating in a monotone "Coo-koo," "Coo-koo." In spite of this behavior he returned to school where he paid no attention to the teacher, other children, or work, but was in and out of his seat, standing on the desk, making writing motions in mid-air, almost mute, never replying to questions; all this without any emotional display except frequent giggling or chuckling to himself. He became restless at night, singing softly to himself while lying in bed and rarely getting to sleep before midnight. He refused to eat unless spoon fed by the mother.

During the six months' period, during which he has been under observation in the institute, silly laughter, grimacing, and blinking have continued throughout. On many occasions he has been observed to attitudinize with his arms partially raised in the air or held at an angle away from the side of the body. At other times he is observed making stereotyped writing motions in the air. He usually carries his head, chin tilted, upward and partially to one side. He will obey commands automatically. He never makes any attempt to integrate himself with the group of children on the ward. He attends school class, does 2B grade work satisfactorily with some individual attention. School work, singing and dancing games in the gymnasium are the only activities thus far which secure his indifferent attention. He will read in a monotonous undertone in an automatic fashion when asked by his teacher. He sings softly to himself and laughs to himself frequently in school class. He is indifferent to his family's visits. He seldom makes an adequate reply to questions and to most questions he replies in a high pitched

much subdued tone of voice with a far off expression, with a stereotyped "somebody," "sometime," "somewhere." His affective tone is one of indifference, disinterest and preoccupation. Mental tests have failed to give anything at all like a satisfactory estimate of his intelligence. The sensorium does not appear clouded. Physically he is of the asthenic type. Neurological examination negative. Endocrine status negative. Laboratory tests and serology negative.

Discussion.—The symptomatology in this case of a general retraction of interests from the environment, stereotyped attitudes and bizarre behavior, episodes of excitement, stereotyped speech, defective emotional rapport, inappropriate affective response, preoccupation, abstraction, and indifference together with a clear sensorium and an adequate intelligence, warrant a diagnosis of schizophrenia of the catatonic form. The onset undoubtedly occurred at the age of four with acute manifestations and was followed by a remission rather than recovery, in which the symptoms persisted, although in a latent form.

In the background is a quiet, unobtrusive, emotionally unresponsive father and an anxious, poorly adjusted, over protective mother. Such personality types may indicate a basic constitutional defect, and again, they may not. The chronic mastoiditis for the first two years of life does not appear to have been accompanied by any indication of an encephalopathy. There is little or nothing to point to an organic substratum in this case, and aside from an asthenic habitus there is little of the constitutional here. Psychodynamically, however, the unassertive father, the over protective mother limiting the patient in his early social contacts, together with the active part played by the mother as the restraining force in the repeated dressings, probably painful, of the mastoid wound, may have had a profound influence on the emotional life of this child, leading to a fixation at a narcissistic pregenital level.

Case III (No. 290).—An Italian boy admitted to the Psychiatric Institute at the age of 9 years and 11 months, because he was disobedient, unmanageable, sleepless and fearful. He is the younger of two children, the sister being a little over a year his senior and of a quiet, shy, conscientious type with normal intelligence. Both parents are reasonably well-adjusted individuals. Relationships between the parents appear to be harmonious. A maternal aunt has dementia præcox. The grandparents reside in the same apartment house on the floor above the patient's family, the grandmother being an extremely moralistic and religious person.

The patient's birth was uneventful, he was a breastfed, well-nourished, happy, contented baby who walked and talked at the usual ages and early acquired proper toilet habits. Except for measles, whooping cough and an occasional earache, he was an exceptionally healthy child with a pleasant, cheerful disposition, yet oversensitive and greatly dependent upon his mother. He was not a gregarious type, yet formed close friendships with two or three boys of about his own age. He spent a great deal of time reading, especially mystery "thrillers," yet took time to play ball and marbles with the children in the neighborhood. He had occasional night terrors, at which times he referred to a "black face" which frightened him. His school record was uneventful, and he was in the fifth grade at the time of admission.

About one year prior to admission, when he was nearly nine years of age, it was noticed that he seemed slowed up, chronically tired, slouched at the table at meal times and became less social. He also became careless about his personal appearance, and spent his whole time when not in school, in reading highly imaginative stories. However, his school work did not suffer until the beginning of the term, two months prior to admission. At about this time he began to grimace and laugh aloud for no apparent cause. He shortly developed episodic outbursts of rage at the slightest provocation, made faces at himself in the mirror, masturbated openly, suddenly running out of the house, referred to "spooks" he had seen, often remarked that "God knows an awful lot" and was unable to verbalize to completion any train of thought, apparently becoming blocked at times, and at other times incoherent. There were times when he seemed to be listening to imaginary voices. His affective state during these few months prior to admission is well indicated by the statement of the father that the patient "has put something up between himself and those talking to him."

During the 15 months' period of observation at the institute, he has been, on the whole, asocial and seclusive. He is usually found in a corner in a comfortable chair reading a book quite unmindful of what is going on about him. The only activity that brings him back to reality for the time being is a game of baseball. He has attended school class regularly, and although he undoubtedly is alive to school-room activities, his attitude is one of a tolerant indifference. He is dilatory at his meals, much of the time seeming quite abstracted. There has never been any spontaneity in his talk but he replies to questions sometimes promptly, sometimes after repeated interrogation and almost always in as few words as possible, yet relevantly but with an air of apathetic abstraction. On occasions he would be observed batting his eyes and tensing the muscles of the lower part of the face into a semi-sardonic grin. On one occasion he said that the figured reindeer on the window drapes moved their lips and could talk to each other but he denied being able to hear them. His affect is characterized throughout by indifference, apathy, abstraction, inadequate emotional responsiveness, and a characteristic lack of emotional rapport. Sensorium clear. I. Q. 100. Physically, he is of average height and weight for his age, with no outstanding physical changes or signs. Neurological examination shows hyperactive deep reflexes throughout as the only sign of any interest. Endocrino-

logically, there are no disorders. He is possibly of the dysplastic build. Laboratory tests and serology are negative.

Discussion.—The clinical symptomatology, marked by a generalized retraction of interest from the environment, asocial behavior, bizarre grimacing, fragmentary evidence of autistic thinking, defective emotional rapport, diminished affective response, together with abstraction, indifference and preoccupation, warrant a diagnosis of schizophrenia of the catatonic form. Such a diagnosis is further substantiated by the tendency to a shut-in personality and the extreme dependence on the mother during his whole life. There appear to be no demonstrable physical factors in the boy's psychosis except the possibility of an intangible constitutional factor as suggested by the dysplastic build and the maternal schizophrenic aunt.

CASE IV (No. 570).—A Jewish boy, 11 years and 2 weeks old when admitted to the Psychiatric Institute, because of impulsive excitement, ideas of reference, suspiciousness and some agitation. He has an introverted, asocial, emotionally blunted, father, who is syphilitic and a somewhat over concerned, neurotic, social, self-pitying, well-educated mother who blames the father for the patient's condition because of the paternal syphilis. The mother has a negative blood Wassermann. Although emotionally the father is a cipher in the home, there is no patent parental discord. Patient is an only child.

The patient's birth was uneventful and he was a well-nourished infant, bottlefed from the age of four months, and teething, walking and talking at the usual ages. He was an unusually quiet baby, not spontaneously happy, who, despite good training, was enuretic until two and a half years of age and was resistant for a time to the usual early attempts directed toward establishing regular toilet habits. He had chicken pox at the age of two and measles at three years, with no significant sequellæ.

At the age of three and a half, he suddenly stopped talking, held himself rigid for periods of several minutes, made faces and showed wide spread choreiform muscular twitchings together with incontinence of urine and feces. These symptoms persisted until the age of five, when they gradually but rapidly disappeared.

At the age of six and a half, he entered the first grade in school and until the time of admission to the institute he made satisfactory progress in school, never having been kept back and seeming better than the average in his subject gradings. He soon developed into an avid reader and delved into archæology and astrology. He never succeeded, nor even attempted, to establish social or play contacts with other children. He often spoke of fearing that other children would laugh at him. He was the shy, "lone-wolf" type of child. Six months prior to admission to the Psychiatric Institute, at the age of ten and a half years, he had an appendectomy and appeared to make an uneventful recovery. He exhibited no untoward emotional

reaction to his illness. While at a resort during his convalescence, he spent much time and showed a great deal of interest in having his fortune told by clairvoyants.

Three months prior to admission he was found one night sitting up in bed, grimacing and saying that his classmates were outside talking about him. The next day he insisted that the windowshades be kept closed. He was sent away to a camp where he kept absolutely to himself, showed no interest in any activities and complained that the boys hated him and talked about him. He suddenly attacked two of the boys and used vile language to the counsellor in an episode of excitement. He was returned home from camp and later refused to return to school, because the boys talked about him, he said, and were jealous of his ability to play the piano and do sculpturing.

During the period under observation at the institute, essentially eight months, his behavior may be characterized as exclusively asocial with episodic periods of rage and destructiveness and unprovoked attacks on other children and nurses, negativism, grimacing registering marked disgust, spitting on his hands and rubbing the saliva on the walls, floors and furniture, and a general retraction of all interest from the environment. He appears at times mildly depressed and unhappy, with periods of prolonged agitated crying. However, his affect is largely one of indifference and apathy with a marked disinterest in his environment and to the visits of his family. At times there is extreme irritation accompanied by cursing the nurses and especially his mother when she visits, with repeated orders on his part to "get out." At times he smiles to himself. Most of the time he is chewing, with an abstracted air, on his tie or other parts of his clothing. He is generally untidy in his personal appearance and indifferent to table manners. In his speech he has been generally underproductive and not spontaneous. At most times his answers to questions are a perseverated indifferent "I don't know" or "yea." On occasions, however, with much interrogation, he said that he did not like the odor of his sputum, that it smelt like urine and that there was a bad odor all around. He retains his saliva in his mouth and is frequently observed assiduously rinsing his mouth at the drinking fountain. He thinks that the other boys on the ward laugh at him. When out walking on one occasion he spontaneously asked the nurse "Why are the cars going so fast, why are they saying: 'Oh, that's terrible'?"

When permitted, he lies in bed with his head covered with the bed clothes, entirely unresponsive and indifferent. Some of his productions in an episode of excitement with no elevation of mood lasting over a period of several days were: "Get out of here you lousy thing." "See the first book of philosophy—11½ years old and his first book of philosophy—a genius—I said—I've written my first book of philosophy, yes, I'm a genius." Later—"Oh, the dirty, slimy, filthy fish." On another occasion he said he was going to make his bones over, that his bones were rotten. Still later "Help me solve my problems." "You lousy father and mother, I'll teach you a lesson. Ah, you wretched mother. I'll show you, you louse, you wretched fucking mother. My bones are rotten, it's my lousy father's fault."

No defect in the sensorium. I. Q. 110.

Physically, he is of the asthenic type, with a slight imbalance of the ocular muscles, widely dilated but reacting pupils; no pathological neurological signs, no endocrine disorders. Serology negative. There is an occasional secondly pubic hair with no other signs of puberty.

Discussion.—The clinical symptomatology characterized by a generalized retraction of interests from the environment, a defect in emotional rapport, dereistic and autistic thinking, perseveration of thought, diminution and distortion of affect, episodes of excitement, negativism with no fundamental mood elevation or depression, bizarre behavior and a clear sensorium, warrant a diagnosis of dementia præcox of the catatonic form. The diagnosis is further substantiated by the presence of a shut-in personality, and a previous attack, similar in its essentials, to the present one, of 18 months' duration. There is considerable reason to maintain that the interval between the first attack which cleared up at five and a half years of age, and the onset of the present attack, which began just prior to the age of 11, was essentially a remission, rather than a recovery, and that the child has had clinical schizophrenia since the onset of the first attack at the age of three and a half years.

Constitutional factors, manifested by the asthenic habitus and the ocular imbalance together with a heredity derived from the constitution underlying the neuroticism of the mother and the schizoidism of the father, plus the possibility of a blastodermic lues, undoubtedly play an important, although indefinable, part in the picture.

Psychodynamically, the dominant part played in the boy's life by the mother and the recessiveness of the father in the family constellation may have made for a persistence of a pregenital level of psychosexual expression with a thwarting of the complete development of the Œdpius situation. With the imminence of puberty, and its attendant physiological surges, the whole unsolved Œdipus situation may have been recapitulated, and the overwhelming pregenital oral erotism and incest motivation thus may have been expressed by way of a psychopathological route.

Case V (No. 25).—A Jewish boy, 11 years and 2 months old on admission to the Psychiatric Institute, presenting irritability, failure in school, and asocial conduct. He is the oldest of four children. Ann, seven years old, is a well-adjusted child; Jack, four and a half years old, gets his own way by insistence and teasing his mother, and Mollie, barely over one year old, is already finding rageful crying effective in producing results. The father

is a man with a classical Hebrew education whose station in life is far below his ambitions, and who has always looked to the patient as the one in whom he could vicariously realize his ambitions. The mother had a mild depression prior to marriage, oscillates between over-indulgence and petting on the one hand and excited scolding and threatening on the other hand, in dealing with her children, while the children almost always eventually gain their point. She is much inferior to her husband educationally, is dissatisfied with the responsibilities of married life, and neither she nor her husband finds in each other a full realization of their emotional needs. She has never freed herself from her own parental ties.

The patient was born after a prolonged labor but there were no indications of intracranial damage at the time. He was a well-nourished infant, bottlefed for three months, who walked and talked at the usual ages. He never had any serious illness. He was restless, fretful and cried a great deal as a baby and soon found this to be successful in gaining attention. He never appeared to be of a sunny happy cheerful disposition but was always a morose, irritable child who put on a temper tantrum usually with head banging, which usually had its desired results from his point of view. He tended to be indifferent to his brothers and sister, except his brother who was six years his junior, for whom he showed some affection. He rarely, if ever, displayed the usual amount of affection for his parents. His whole life was marked by a generalized indifference. He rarely, if ever, asked for a new toy and if one was presented to him, he accepted it with stolid indifference. Since early childhood he would have periods of marked sullenness and obstinacy. At times he would scream for hours because a wish was thwarted. On occasions he would stand outside the house by the door, refusing to come in.

He started to school at the age of six years and made regular progress until about two years prior to admission, when in the fourth grade, he habitually failed because, the teacher thinks, he lost all interest in school work and seemed absorbed in his own thoughts. He never played with his school mates or showed any interest in them. He would come home immediately after school and spend his time sitting about the house reading a book or simply idling about. About two years prior to admission, when about nine years of age, his sullenness and irritability increased. His talk became so incoherent at times that it was not possible to understand him. He would have outbursts of anger, laughter and grimacing. At times he would refuse to eat. He began complaining of things crawling on his body. He complained that other children made fun of him. He called his mother vile names in fits of anger, would stand for hours before the mirror, stayed in the bath room for prolonged periods, sat by himself in the dark, and some times disappeared from home and would be found in a nearby movie.

During the first 12 months of his stay in the institute, he spent the major part of the time gazing fixedly out of the window with a perplexed frown on his face, and at times would be found under a bed or crouched in a corner behind a piece of furniture. At times there would be periods when he became quite excited and would attack the other children. He mechanically

attended school and occupational therapy, but showed little interest. His speech at times was voluble but irrelevant, disconnected and full of neologisms. His replies to most questions were a stereotyped, colorless "I don't know." There was no outstanding mood deviation and his affect was characterized by a combination of disinterest, detachment and perplexity. He sometimes laughed long and loudly for no obvious reason. He spontaneously referred to voices at times but one could not learn their content. Some of his spontaneous productions were as follows: "Do you see the bone" (indicating left shoulder). "I slept on it and now my bowels moved out through it." "I have a red pulse down my neck." "I never breathe." "Look down my throat and you can see a red bone." "Every second the pulse runs through." "I have red pulse sores." "The inside of my skin is bitten," etc., etc. His sensorium remained clear throughout. A psychometric examination proved unsatisfactory.

After approximately a year in the hospital, he began to show an improvement. His interest was mainly intrigued by occupational therapy at first and then spread to persons about him, especially the nurses. He frequently complained, without cause, of the other boys making fun of him and disliking him. He became more communicative in his talks with the physician and displayed an active interest in the possibility of going home. After a period of 18 months in the hospital he went to a camp for the summer where he made a reasonably good adjustment in a negative, colorless, fashion. He showed a passing interest in the sports but usually kept to himself. He returned home and entered school where his adjustment, at the best, has been a mediocre one. The present indications are that he will, sooner or later, again require hospitalization. Physically, he is of the dysplastic type with no evidence of puberty. Endocrinologically, there is nothing of note and the neurological examination is negative. Serology negative.

Discussion.—The symptomatology in this case is classically that of schizophrenia. The age of onset is difficult to determine, yet it seems certain it was present at the age of nine years, if not before, and that it developed on a typical schizoid type of personality. What, if any, are the constitutional factors, is difficult or impossible to indicate. Possibly, whatever they may be, they were largely a contribution from the unstable, highly neurotic mother. The oscillating attitude of the mother toward him, the early discovery by himself of the value of certain emotional behavior in securing his satisfactions, and the centering upon himself of the father's thwarted ambitions and the mother's unsatisfied emotions, may have created within him a feeling of insecurity and uncertainty. Psychodynamically, he shows undoubted evidences of having failed to progress much beyond a narcissistic level and to have found his satisfactions in a world largely within himself.

CASE VI (No. 188).—A Hebrew boy, admitted to the Psychiatric Institute at the age of 12 years and 3 months because of failure in school, sleeplessness, refusal of food, episodic excitement, and a generalized lack of interest. The patient is one of seven children; two completed high school and have well-balanced personalities; one is struggling through high school and is regarded as somewhat of a dreamer, unfriendly and at times irritable; another is cheerful, social, happy, adolescent doing good work in high school; the fifth was said to be a normal child who died of an acute illness at the age of 11 years. The patient is the sixth born and there was, after him, a girl, one year his junior, who died of acute appendicitis at the age of seven years and to whom the patient was much attached. Both parents are of a mediocre level educationally, the mother is a well-balanced, sensible person, but the father is extremely emotional in times of stress. Both parents seem happily mated.

The patient's early history is entirely uneventful and free from any indication of physical or mental difficulty. He had been regarded, up to the time of his present illness, as a happy, cheerful, fun-loving child, well liked by other children, offering no special problem of any kind. His school history was not outstanding in any way until his present condition had been established for nearly a year, at which time he had reached the fifth grade.

When the patient was eight years of age, his sister one year his junior, with whom he was quite companionable, died. Shortly after this he appeared unduly quiet and given to worry. He began to complain of shortness of breath, pains in his side and seemed apprehensive and anxious about himself. These symptoms cleared up after a period of six months, but thereafter he never again mingled or played with other children, spending most of his time in the house and seeming subdued and at times abstracted. When he was 11 years of age, one year prior to admission, a death occurred in the same apartment house and was colored by much audible wailing and mourning of the deceased's family. The patient's physical complaints and anxieties thereupon reappeared and he said he had a hole in his heart and that all his bones were broken. He became fearful of going to bed, stayed up to all hours of the night and would not go out alone. He frequently took off his shirt to examine himself carefully, He began to refuse food, would not allow himself to be washed or dressed, became antagonistic toward his mother and had frequent outbursts of rage. The more acute manifestations cleared up in a few months and he was sent away to a boarding school. There, he at times had to be forcibly fed, was neglectful of his personal appearance, seemed abstracted, daydreamed and did not pay any attention to the other boys. After a period of about one year in the school, he returned home and was later admitted to the Psychiatric Institute.

On admission to the Psychiatric Institute he seemed completely self-absorbed and reacted continuously to auditory and visual hallucinations. There were frequent periods of excitement colored by an admixture of fear and rage, apparently in relation to his hallucinatory experiences, in which he would be impulsively aggressive. He showed no interest in the other children, spent most of his time gazing out of the window, and seemed

suspicious and a bit fearful of everyone approaching him. Sometimes he would stand before a mirror, grimacing. His replies to most questions were a stereotyped "I don't know." At times he would talk spontaneously though incoherently. Some of his productions were: "I know what's the matter with me. It's the nail she hammered in my foot. I told her not to do it. Take a bath, fall asleep next day. I don't know what comes of me. Oh, God, if the ambulance comes it will be good. I forget my sister already." There was no essential depression or elevation of mood. His affect has been characterized throughout by indifference, inconsistency and apathy. His sensorium was clear.

During the 18 months' period of observation in the institute, there have been periods in which he would show some improvement, take sort of a detached interest in his surroundings and be fairly cooperative. At other times he would be irritable, impulsive, and entirely absorbed in a world of fantasy. Throughout, his mood has shown no appreciable change. His hallucinations and delusions have been present continuously and the sensorium has remained clear. Psychometric tests have not been feasible but the general impression is that he is a boy of at least average intelligence. Physical examination shows a boy, well-nourished, of the pyknic habitus with a tendency to increased subcutaneous fat about the abdomen, breasts and hips which might be interpreted by some observers as evidences of a pituitary insufficiency. There are no evidences of puberty. X-ray of the skull is negative. Neurological examination shows nothing of a significance. Serology negative.

Discussion.—The symptomatology in this case is typical of schizophrenia. Constitutionally, there are an emotionally unstable father in the background and a suggestion of an endocrine imbalance in the patient involving the pituitary. On the emotional or psychogenic side is the strong attachment between the patient and his sister whose death marked the onset of the psychotic manifestations at the age of eight years. The absence of a schizoid personality in his early childhood is of interest and may portend a fair prognosis in spite of the malignant characteristics of his psychosis. Psychodynamically, regression to a narcissistic level is not as clear as in some of the other cases in this series and this in itself may be of prognostic significance. There are some indications that we are dealing with a prolonged incest panic concerning itself with the dead sister, who probably represents the mother in his unconscious.

Conclusion.

From this study it appears that a typical schizophrenic reaction may put in its appearance long before the initiation of pubescence.

Because of the fact that the child is limited in his verbalizations and the fact that his thinking is in the direction of concreteness rather than abstraction what little delusional formation there is, is exceedingly simple and naive. The outstanding symptomatology is found in the field of behavior and a consistent lack of emotional rapport. The drive for integration with the environment, so characteristic of normal children and so essential for their personality development is outstandingly absent.

The children in this report show only occasional traces of projected narcissism or homoerotism, and on account of their age alone, heteroerotism is not expected to be manifested. Their libido is almost exclusively invested in narcissism and autoerotism.

Although this communication is not concerned with the discussion of the treatment of schizophrenia in children, it may be stated as a matter of information that none of the cases reported have shown any outstanding improvement over the period of hospitalization, ranging from 3 to 18 months. The treatment thus far has been that of continuous effort to intrigue the child's interest through a carefully organized daily routine, modified to fit the individual, consisting of individual nursing attention, school, occupational therapy, special movies, organized play, etc. It has not been possible to do a great deal with a direct psychotherapeutic approach because of the lack of an essential emotional rapport. At some future date we expect to report in considerable detail our further studies regarding treatment.

There is a superficial resemblance of schizophrenic children to certain so-called unstable mental defectives. The schizophrenic child often appears mentally deficient because the libido is invested within the patient himself, thus interfering with the objectification of the intellectual processes. It is the thought of the writer that a careful psychiatric study, from a psychodynamic approach, of the patients in institutions for mental defectives, might demonstrate that schizophrenia in children is not as rare as is now generally believed.

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DISCUSSION.

Dr. Henry I. Klopp (Allentown, Pa.).—Dr. Potter's paper emphasizes very definitely the importance of studying children not only during the school but also the pre-school age. His closing statement beyond question is correct, wherein he says he feels that many of these children have been overlooked in the past, not properly interpreted and merely considered as cases of mental deficiency.

Our experience at Allentown coincides somewhat with that of Dr. Potter. We have not seen the cases which he has emphasized, namely, those of the first decade. We have had four children comparatively recently over ten years of age. All these are somewhat similar to those presented by Dr. Potter, although no two, however, are alike.

For example, resistiveness, indifference, self-absorption or what might possibly be regarded as confusion with outbursts of active resistance and hallucinations. The following is a brief synopsis of two out of four cases:

The first, a girl 13 years of age had been in a private institution and was later, for financial reasons, transferred to the Allentown Institute for Children. She was diagnosed by us as dementia præcox of the catatonic type. The picture she presented in the beginning was regarded as one of hysteria. The family history is exceedingly interesting. The father is said to have had a nervous breakdown from which he recovered. Several paternal relatives were nervous and superstitious. The mother is subject to fainting attacks but denies that these are epileptic. She admits that she does not properly discipline her children. In fact, from our observation we are satisfied that the father and mother are both of a low intellectual level. One sister does not apply herself in school.

Birth was instrumental but her development was normal. At the age of 12 she entered junior high school. She was frightened by the onset of menstruation. In the beginning of her mental symptoms, she walked stiffly and assumed awkward positions. She had temper tantrums, and was inclined to be absent-minded, then began sitting around for hours without saying a word. On admission, she was mute, kept her eyes closed and teeth tightly approximated. She remained in this position for weeks not changing her position and taking no interest in anything whatsoever. She was finally given carbon dioxide with oxygen inhalation. She also received physiotherapy, especially ultra-violet treatments, and particularly individual nursing, which included suggestion. She improved gradually, was able to get about and took an interest in things around her. She was finally permitted to go home for a week's visit with her parents. They were persistent about this, and it did not surprise us when they failed to return her. Unfortunately, we have not been able to keep in touch with this case.

The second patient, that of a boy who at the age of onset, was 10 years old. He was also diagnosed as dementia præcox of the catatonic type. The father was alcoholic.

The history of the patient was that he was a behavior problem and finally, due to thieving, he was committed to Glen Mills, spoken of commonly as a reform school. Soon after his arrival at this institution, he threatened to commit suicide, and he would sit in a corner, would suddenly get up, rush out and yell. He attempted suicide and told other boys that he wished to kill them. He banged his head against the wall and said the devil made him do this.

He has had periods of mutism, admitted hearing voices of invisible people. He had periods of improvement under treatment with relapses. More recently, he began to improve, cooperates well in occupational therapy activities, attends school and shows interest in reading. With it all, however, he is inclined to keep by himself.

The other two cases which we have had are somewhat similar to those cited and present a very clear picture similar to those presented by Dr. Potter.

I do believe the more attention we pay to these so-called problem children, study and analyze the onset of their mental symptoms including their home environment, the more interesting our psychiatric studies of children will become.

CHANGES IN THE CLINICAL SIGNS AND LABORATORY FINDINGS IN VARIOUS TYPES OF PSYCHOSES UNDER THE INFLUENCE OF SUBCUTANEOUS ADMINISTRATION OF OXYGEN.*

By J. NOTKIN, M. D., J. G. WILLIAM GREEFF, M. D., F. H. PIKE, PH. D., AND J. A. KILLIAN, PH. D.

Oxygen as a therapeutic agent has been used for the last 20 years and especially during the World War in certain pyogenic infections. Beneficial effects from oxygen have been claimed in such conditions as pulmonary tuberculosis, anemias and cardiovascular involvement. The French psychiatrists have been using oxygen in depressions both in the form of inhalations and subcutaneous insufflations for years (Heckel). They report good results. About 15 years ago one of us attempted oxygen therapy in a small group of involution melancholia patients. This selection of cases was based upon the chronic course of depression evident in these patients. The results at that time were encouraging. Critical study of the case histories, however, indicated that some of the treated patients belonged to the benign group of Hoch and MacCurdy and that the others were of the manic-depressive type. Growing interest in gaseous therapy in psychiatry in recent years suggested that another attempt with oxygen therapy should be made on a larger scale with various types of patients with the objective of evaluating this form of treatment, adopting as criteria the various clinical signs and laboratory findings for metabolic changes under the influence of oxygen.

In this investigation four groups of patients have been selected for oxygen therapy. The first group includes 30 female patients with involution melancholia. As the admission rate of this type of patient is relatively small, it was necessary to take all the patients available who cooperated sufficiently to make them suitable subjects for an investigation. Both the malignant and benign types are represented. They were all diagnosed by the hospital staff at the

* Read at the eighty-eighth annual meeting of The American Psychiatric Association, Philadelphia, Pa., May 30–June 3, 1932.

regular staff meetings. It was not possible to make a simultaneous study of control cases nor to have an equal number of benign and malignant cases since the available material was rather restricted.

The second group consists of 8 cases of mixed manic-depression and 2 cases of inadequacy depression. The frequency of unfavorable prognostic features in the mixed manic cases and the relatively long residence in the hospital of the two cases of inadequacy depressions were the bases for inclusion of these patients.

In the third group are 10 cases of catatonia of long duration. Here patients showing pronounced cyanosis of the extremities were selected.

The last group included 7 patients with convulsive states. The reason for including this type of patient in our investigation was to ascertain whether an increased oxygen content in the circulating blood would exert any effect on the frequency of the convulsions, since Spielmeyer has hypothecated the occurrence of anoxemia of the brain during a convulsive attack. The relatively small number of patients in this group is explained by the fact that it was necessary to discontinue routine treatments of these patients in order to avoid the effects of bromide or luminal and also because the cooperation of only a small number was secured.

Originally the plan of the investigation was limited to the changes of the cytology of the blood, the basal metabolic rate and body weight under the influence of this therapy. As the study progressed new problems presented themselves and interest arose in the gas changes in the venous blood (oxygen capacity, oxygen content, oxygen unsaturation and CO_2 content). Further it appeared possible that variations in the pulse rate and blood pressure under the influence of the therapy might possess some interest. In view of the gradual enlargement of the scope of the investigation, some of the above-mentioned details are lacking in the reports of a number of patients. Determination of gas changes in the blood especially, could not be made on all cases, owing to the practical difficulties of the technic.

After a preliminary study of each individual patient, including complete physical and mental examination, recording of the blood pressure, pulse rate, weight, and basal metabolism, blood counts and gas analysis of the venous blood, a bi-weekly administration of oxygen in the form of subcutaneous insufflations was begun. The

first insufflation in each case was 500 cc., while all subsequent treatments were 1000 cc. On the same day weight, pulse rate and blood pressure were again recorded. Other analyses such as determination of basal metabolic rates made at longer intervals. After a course of treatment from 3 months to almost a year, a second, complete examination of the patient was made with a check-up on the various laboratory findings. A graphic representation of all the changes occurring in all the patients during the course of the treatment is impractical, and only the initial and final results in each patient are tabulated in the form of percentages for the group.

Specimens of blood were taken under basal conditions. This is an important factor in the evaluation of the number of white blood cells. The gas analyses of the blood were carried out at the biochemical laboratory of the New York Post Graduate Hospital, using Van Slyke's manometric apparatus. The other tests and examinations were made at Manhattan State Hospital. In order to present the results graphically the various findings were tabulated in the form of comparative tables. The findings in each group have been presented separately, and an attempt will be made to correlate the various groups.

Involution Melancholia.

1. *Cytology of the Blood.*—In a group of 30 patients prior to treatment a low hemoglobin was found in 20 per cent of the cases and an average normal in 80 per cent. After a course of treatment, check-up analyses on 27 patients showed only 4 per cent with low hemoglobin content, and an increase in the number of cases with normal findings to 96 per cent. The observed changes after therapy were as follows: in 48 per cent an increase, in 33 per cent a decrease, and in 19 per cent no change in the hemoglobin content. After classification of the cases in relation to their mental condition it was found that in the improved group of 13 patients, 50 per cent showed an increase, 17 per cent a decrease, and 33 per cent no alteration in the hemoglobin content; and in the unimproved group, 47 per cent showed an increase, 40 per cent a decrease and 13 per cent unchanged hemoglobin content.

To summarize these findings it may be said that prior to treatment an average normal hemoglobin content was found in the great majority of the cases and that after treatment there was an appreciable increase in the total number of cases with normal hemoglobin. In general, the improved group showed more benefit in this respect than the unimproved.

Predominantly normal values for erythrocyte counts were found in this group with no marked alterations during treatment, nor was there any correlation between mental improvement and change in red cell count.

As previously described, the specimens of blood were taken under basal conditions and, a white blood cell count of 8000 per c. ml., as about the normal limit. In this group of cases, the majority exceeded this limit, and treatment did not decrease but rather increased the leucocyte count with the exception of the slight diminutions observed in patients benefited by the treatment. In these cases, the white blood cell count was decreased coincidently with treatment. The polymorphonuclear cell count was normal, or low initially, with appreciable change during the period of treatment. The lymphocytes however showed a large proportion of high counts, with no conspicuous change in either direction after treatment. Normal counts were found both before and after treatment for the basophiles, with no change during the period of observation. The endothelial cells were also normal both prior to and after a course of treatment, with a slight tendency toward a decrease. The eosinophile count was in all cases within normal limits, but the improved patients exhibited a tendency towards an increase after therapy. In general, there was no marked change in the white blood cells of patients with involution melancholia during oxygen therapy, although the total count was high with a high proportion of lymphocytes. The thrombocyte count was normal or low throughout, with practically no change due to therapy.

2. *Autonomic Signs.*—Determinations of basal metabolic rate were made in all cases of involution melancholia prior to the course of treatment, and in all but three cases after treatment.

In 57 per cent of the cases the initial basal metabolic rate was within normal limits (10 per cent above to 10 per cent below average normal); in 30 per cent it was below normal, sometimes as low as −28; in 13 per cent it was high, reaching 20 per cent above

average normal. It is important to note that the high readings were always found in agitated cases, even in the absence of outward motor agitation. The abnormally low readings were noted in the affectless types or in cases with little or no agitation, principally in depressions. After a course of treatment an increase in the proportion of cases with higher rates was found especially due to an increase in the readings for the improved patients. There was no dominating direction of change in the unimproved cases.

The systolic blood pressure prior to treatment ranged from 98 to 165, the majority of the readings lying between 100 and 150. The average systolic blood pressure seemed to be rather low considering the facts that the patients are in the late forties and middle fifties, that is, in a more or less organic period. After treatment variations in the blood pressure were observed in both directions, with perhaps a prevailing tendency toward a decrease. Frequently many patients were observed who showed higher readings during states of agitation than during quiescent periods. Analysis of the cases before treatment shows that 54 per cent of the cases improved by the treatment had initial systolic blood pressure between 121 and 151, while 38 per cent were in the 101 to 120 group, and the remaining 8 per cent in the group with pressure of 100 or less. However of the cases exhibiting no improvement, 53 per cent had systolic blood pressures between 101 and 120, while 24 per cent were 100 and less, 18 per cent 121 to 150, and one case showing a pressure above 150. These results indicate that the patients responding to the treatment clinically were those with a blood pressure initially higher than in those who showed no response. After treatment, however, approximately one-half of the patients in both groups showed a drop in blood pressure. The significance of these changes in blood pressure in involution melancholia during oxygen therapy is not evident.

In the majority of cases before treatment, the pulse rate varied from 71-100; the minority had pulse rates lower than this level. The extremes were 56 and 122 in the preliminary examinations. During the course of treatment a definite rate decrease was noted in the clinically improved patients.

Weight was taken in all cases before, during, and at the cessation of treatment. The extremes were 69 pounds and 175 pounds, with 70 per cent of the figures between 69 and 125 pounds. Cases

of involution melancholia in general are undernourished. During treatment there were fluctuations in both directions, with some predominance in the direction of increase. This was not correlated with mental improvement.

3. *Gas Changes.*—Analyses of blood gases were made in 19 of the 30 cases, but in 3 of these a post-therapeutic check-up could not be made. Analyses on a series of normal subjects have yielded 17.5 to 21.4 volumes per 100 ml. as normal figures for the oxygen capacity of blood. These limits agree well with the findings reported by Williamson, Haden and Appleton for normal adult females. The majority of the cases in this series lie within the normal range both before and after treatment, but the final figures for the improved cases often showed a drop whereas the unimproved cases as frequently showed a rise. These changes are not in accord with the described changes in hemoglobin, and they are sufficiently great to bring the majority of the readings out of the normal range, and can probably be accounted for by the limits of experimental accuracy as well as the range of normal variation in the same individual. On the whole both groups show similar proportions of normal values with occasional low figures for oxygen capacity.

Normal figures for the oxygen content of venous blood lie between 9.5 and 17.9 volumes per cent, within the experience of this laboratory, agreeing well with the figures reported by Lundsgaard. This indicates a saturation of the venous blood with oxygen to the extent of 60 to 85 per cent. The results in the cases of involution melancholia were normal or low, with a very slight tendency to decrease after oxygen therapy. This result of doubtful significance was not correlated with mental improvement.

Assuming the normal figures for oxygen unsaturation to range from 2.7 to 8.9 volumes per cent, the figures for the series were normal or high, with a tendency toward an increase after treatment in those cases showing improvement.

The coincidence of low figures for oxygen content with normal or slightly decreased figures for oxygen capacity (and of hemoglobin) excludes anemia as a cause of the observed anoxemia. Oxygen capacity and hemoglobin vary together and are both low in anoxemias of anemic origin. In stagnant anoxemias, however, a low oxygen content is found since the sluggish flow through the tissues permits greater removal of oxygen from the blood.

An abnormal demand for oxygen from the blood resulting from an acceleration of oxidation reactions in the body may produce a rise in the oxygen unsaturation of the blood. However, under such conditions a high basal metabolic rate is to be expected. The frequency of normal or sub-normal basal metabolic rates excludes increased rate of oxidation reactions as the factor effecting the abnormal oxygen unsaturation of the blood. Sluggish capillary flow may be a more plausible explanation for the initial oxygen findings in these cases.

During treatment the values for basal metabolic rate and oxygen unsaturation change in the same direction in 12 cases, while in only 3 cases do they change in opposite directions. Furthermore, these relationships are not correlated with improvement, nor are they either increased or decreased predominately by treatment. The changes observed are due probably to other fortuitous factors during the period of therapy.

The average normal figures for CO_2 content of blood are from 48 to 55 volumes per cent. Both before and after treatment 63 per cent of the cases give normal values, while in the remaining 37 per cent high figures were noted. The findings for CO_2 content agree well with those for oxygen content since it is to be expected that they would be reciprocal. Christiansen, Douglas and Haldane found that when oxygen combines with hemoglobin of partially reduced blood, it drives out a certain amount of CO_2. Since this reaction is reversible, a high oxygen unsaturation with a low oxygen content should be accompanied by a high CO_2 content, which is the case in the present series of those patients with readings outside the normal range.

4. *Psychiatric Considerations.*—As stated in the introductory remarks, there is a reasonable assurance of correct diagnosis inasmuch as the patients were seen by several observers who agreed with the classification. The cases who showed improvement were again seen by various men on the occasion of their discharge from the hospital. In the classification of the cases we were guided by the Hoch and MacCurdy conception of the benign, malignant and indefinite types. It would be superfluous to repeat here the differences in the various groups and as citation of detailed case histories would hardly serve the purpose, we shall limit ourselves to the tabulation of certain findings.

INVOLUTION MELANCHOLIA.

30 Cases—13 Improved.

HEMOGLOBIN.

Condition.	Per cent of cases. Normal.	Low.	High.	Condition after treatment.	Per cent of cases. Increased.	Decreased.	Unchanged.
Before treatment	80	20	..	Total	48	33	19
				Improved	50	17	33
After treatment	96	4	..	Unimproved	47	40	13

RED BLOOD CELL COUNT.

Before treatment	77	13	10	Total	44	56	..
				Improved	42	58	..
After treatment	92	4	4	Unimproved	47	53	..

WHITE BLOOD CELL COUNT.

Before treatment	37	..	63	Total	52	44	4
				Improved	33	58	9
After treatment	30	..	70	Unimproved	67	33	..

POLYMORPHONUCLEARS.

Before treatment	43	47	10	Total	48	41	11
				Improved	42	50	8
After treatment	59	41	..	Unimproved	53	33	14

INVOLUTION MELANCHOLIA.—CONTINUED.

LYMPHOCYTES.

Condition.	Per cent of cases. Normal.	Low.	High.	Condition after treatment.	Per cent of cases. Increased.	Decreased.	Unchanged.
Before treatment	63	..	37	Total	48	44	8
				Improved	50	42	8
After treatment	56	..	44	Unimproved	47	47	6

BASOPHILES.

Before treatment	100	..	..	Total	15	15	70
				Improved	17	8	75
After treatment	100	..	..	Unimproved	13	20	67

ENDOTHELIAL CELLS.

Before treatment	100	..	..	Total	26	44	30
				Improved	25	50	25
After treatment	100	..	..	Unimproved	27	40	33

EOSINOPHILES.

Before treatment	100	..	..	Total	48	19	33
				Improved	67	..	33
After treatment	100	..	..	Unimproved	33	33	33

THROMBOCYTES.

Before treatment	43	50	7	Total	58	42	..
				Improved	50	50	..
After treatment	29	54	17	Unimproved	62	38	..

INVOLUTION MELANCHOLIA.—CONTINUED.

BASAL METABOLISM.

Condition.	Per cent of cases. Normal.	Low.	High.
Before treatment	57	30	13
After treatment	52	26	22

Condition after treatment.	Per cent of cases. Increased.	Decreased.	Unchanged.
Total	56	44	..
Improved	73	27	..
Unimproved	44	56	..

	Toward normal.	Away from normal.	Normal before and after.
Total	30	33	37
Improved	26	18	56
Unimproved	39	44	17

BLOOD PRESSURE.

	100 and less.	101-120	121-150	151 and more.
Before treatment	17	37	33	13
After treatment	27	37	23	13

Total	40	50	10
Improved	31	46	23
Unimproved	47	53	0

PULSE RATE.

	70 and less.	71-100	101 and more.
Before treatment	27	64	9
After treatment	36	56	9

Total	32	64	4
Improved	0	86	14
Unimproved	47	53	0

INVOLUTION MELANCHOLIA.—Concluded.

WEIGHT.

Condition.	Per cent of cases.			Condition after treatment.	Per cent of cases.		
	Normal.	Low.	High.		Increased.	Decreased.	Unchanged.
	100 and less.	101-125	126-150 / Over 150.				
Before treatment .. 37	33	23	7	Total	59	38	3
				Improved	50	42	8
After treatment ... 38	34	17	10	Unimproved	65	35	..

OXYGEN CAPACITY.

Before treatment	84	11	5	Total	44	56	..
				Improved	30	70	..
After treatment	75	25	0	Unimproved	67	33	..

OXYGEN CONTENT.

Before treatment	53	47	..	Total	44	56	..
				Improved	40	60	..
After treatment	44	56	..	Unimproved	50	50	..

OXYGEN UNSATURATION.

Before treatment	42	..	58	Total	62	38	..
				Improved	70	30	..
After treatment	44	6	50	Unimproved	50	50	..

CO_2 CONTENT.

Before treatment	63	..	37	Total	50	50	..
				Improved	60	40	..
After treatment	63	..	37	Unimproved	33	67	..

Of the 30 cases observed 13 responded favorably to the therapy and were all released from the hospital. Of the remaining 17 two died during the course of treatment and the rest are still in the hospital. Of the 13 improved cases 11 were definitely considered as benign. They presented the typical picture of involution melancholia but none of them exhibited peevishness or affectless states and the other signs which Hoch considered as malignant features. The two other cases were decidedly malignant. One of the patients was not only affectless but practically mute and the other one exhibited considerable peevishness.

In the group of unimproved cases there were 14 which were considered by all observers as of the malignant type. The remaining three impressed everyone as benign cases and favorable prognostications were made. However, none of those patients has improved as yet, almost 2 years since the therapy was instituted. Two of the patients of this group have died. Before we consider the other factors we should like to emphasize the fact that on the whole the Hoch and MacCurdy classification of involution melancholia cases is in all probability a correct one and has been borne out by our own material. The few cases in each group which do not seem to tally with Hoch's classification may belong to the group which he classified as indefinite, and further observation may throw more light on their real nature.

We have tabulated a few other factors in order to ascertain their relationship to the final outcome of the illness. Of these, we have especially considered the age of the patient, duration of illness and of treatment. The ages of the patients at the beginning of treatment ranged from 44 to 63 in the unimproved group and 39 to 62 for the improved group. The average age in the unimproved group was 53 years and in the improved 52, the average of all the cases being 52.5 years. Of considerable interest is the duration of the illness up to the time of treatment. In the improved group we find the average length of the disease to be 3.5 years whereas in the unimproved group the average is 5.3 years, a difference of almost 2 years. This may indicate that if there is any value in oxygen therapy at all, it is important to institute it as early as possible in the course of illness. The duration of treatment does not seem to stand in any direct relation to the effects therefrom. We have seen patients improving after a rather short period of treatment of 3 months

and other patients show no improvement, after almost 12 months of continuous treatment. The average duration of treatment in the improved group was 5 months, in the unimproved 6.2 months, and for that matter could have been continued longer with the same lack of results. We found further than in the improved group the benign cases comprise 85 per cent and the malignant 15 per cent of the patients. In the unimproved group 17 per cent were benign cases, 77 per cent malignant and the remaining 6.0 per cent the so-called indefinite cases.

Manic-Depressive Psychosis.

As stated in the introduction, the group of manic-depressive psychosis included 8 cases of mixed manic psychosis and 2 of inadequacy depression. Only 2 cases of this group improved, 1 of each type.

1. *Cytology of Blood.*—The hemoglobin content of the blood was, on the whole, normal, with a few low findings. No definite change was evident during the period of treatment. The erythrocyte count was normal, both improved cases showing an increase after therapy. The group as a whole however, showed a decrease as frequently as an increase.

Prior to treatment, the majority of the patients had unusually high leucocyte counts, reaching 17,000 in some instances. In both improved cases decreases were found after treatment, one, from 17,000 to 9400 cells per c. mm., but a similar change was noted in only 38 per cent of the remaining cases. Although these decreases were not consistent, it must be noted that the maximum leucocytosis after treatment was below the level of 13,000 per c. mm. The polymorphonuclear count was either normal or low, with fluctuations equally in both directions during the period of observation. The lymphocytes were either normal or high in number and there was no characteristic change observed after treatment. Normal values were obtained both initially and finally for basophiles, endothelial cells and eosinophiles in all cases. There was observed no change in the basophile count, but both improved cases showed an increase in endothelial cells and decrease in eosinophiles. The unimproved cases showed a decrease in endothelial cells and no definite change in eosinophiles.

The thrombocytes in the majority of cases were within the normal range, and a decrease was noted after treatment in all except one improved case.

2. *Autonomic Signs.*—Determinations of basal metabolism were made on all cases before and after treatment. Before therapy only 20 per cent of the cases gave low values, the lowest being 23 per cent below average normal, all others were within the normal range of ±10 per cent. Two improved cases gave normal values both before and after, but the other cases showed a tendency to decrease, the lowest to 25 per cent below average normal.

Sixty per cent of the group had systolic blood pressures of 100 or less both before and after treatment. The readings for both improved cases increased during treatment, from initial levels of 80 and 86, but there was no consistent change evident in the rest of the group. No significant variations were noted in the pulse rates.

Body weights of the majority of patients was less than 125 pounds, although one was observed to weigh 274 pounds. Both improved cases showed slight increases in weight, but the remaining patients in general exhibited an insignificant decrease in weight. On the whole, the nutritional state of this group was below par and treatment produced no demonstrable improvement.

3. *Changes in Blood Gases.*—In only 3 cases was it possible to analyze the blood for oxygen and CO_2, and in only 2 of them, neither showing any improvement, was a post-therapeutic check-up possible. The very small number of cases must be kept in mind in the interpretation of the results. Results for oxygen capacity showed a normal, a low and a high value. In both cases checked after treatment a decrease in capacity was noted. Oxygen contents were low initially and increased during treatment, but the increase was insufficient to raise it to the normal range. High values were obtained for oxygen unsaturation, with a decrease at the end of the period. The CO_2 content was normal or high and showed no characteristic change in the 2 cases observed.

REMARKS.

There is no need for lengthy discussion on the psychiatric aspect of these patients as they showed the typical findings of the group. We feel, however, that the following may be of some interest. The

MIXED MANICS.

10 Cases—2 Improved.

HEMOGLOBIN.

Condition.	Per cent of cases.			Condition after treatment.	Per cent of cases.		
	Normal.	Low.	High.		Increased.	Decreased.	Unchanged.
Before treatment	70	30	..	Total	30	40	30
				Improved	50	..	50
After treatment	80	20	..	Unimproved	25	50	25
RED BLOOD CELLS.							
Before treatment	90	10	..	Total	60	40	..
				Improved	100	..	..
After treatment	100	..	..	Unimproved	50	50	..
WHITE BLOOD CELLS.							
Before treatment	20	..	80	Total	50	50	..
				Improved	..	100	..
After treatment	30	..	70	Unimproved	62	38	..
POLYMORPHONUCLEARS.							
Before treatment	50	..	..	Total	50	50	..
				Improved	50	50	..
After treatment	50	..	10	Unimproved	50	50	..

MIXED MANICS.—CONTINUED.

LYMPHOCYTES.

Condition.	Per cent of cases. Normal.	Low.	High.	Condition after treatment.	Per cent of cases. Increased.	Decreased.	Unchanged.
Before treatment	50	..	50	Total	60	40	..
				Improved	50	50	..
After treatment	40	..	50	Unimproved	62	38	..

BASOPHILES.

Before treatment	100	..	..	Total	10	20	70
				Improved	..	50	50
After treatment	100	..	..	Unimproved	15	15	70

ENDOTHELIALS.

Before treatment	100	..	..	Total	30	70	..
				Improved	100	..	..
After treatment	100	..	..	Unimproved	12	88	..

EOSINOPHILES.

Before treatment	100	..	..	Total	30	50	20
				Improved	..	100	..
After treatment	100	..	..	Unimproved	37	37	25

THROMBOCYTES.

Before treatment	75	13	12	Total	13	75	12
				Improved	50	50	..
After treatment	20	70	10	Unimproved	..	83	17

MIXED MANICS.—CONTINUED.

BASAL METABOLISM.

Condition.	Per cent of cases. Normal.	Low.	High.
Before treatment	80	20	..
After treatment	50	50	..

Condition after treatment.	Per cent of cases. Increased.	Decreased.	Unchanged.
Total	50	50	..
Improved	100	..	..
Unimproved	37	63	..

	Toward normal.	Away from normal.	Normal before and after.
Total	20	40	40
Improved	..	..	100
Unimproved	25	50	25

BLOOD PRESSURE.

	100 and less.	101-120	121-150	151 and more.
Before treatment	60	20	20	0
After treatment	60	30	..	10

	Per cent of cases. Improved	Unimproved	Unchanged.
Total	50	30	20
Improved	100	..	..
Unimproved	38	37	25

PULSE RATE.

	70 and less.	71-100	101 and more.
Before treatment	25	63	12
After treatment	25	38	37

	Per cent of cases. Improved	Unimproved	Unchanged.
Total	50	38	12
Improved	100	..	..
Unimproved	33	50	17

MIXED MANICS.—CONCLUDED.

WEIGHT.

Condition.	Per cent of cases.				Condition after treatment.	Per cent of cases.		
		Normal.	Low.	High.		Increased.	Decreased.	Unchanged.
	100 and less.	101-125	126-150	Over 150.				
Before treatment ..	40	40	10	10	Total	40	60	..
					Improved	100	..	..
After treatment ...	20	70	..	10	Unimproved	25	75	..

OXYGEN CAPACITY.

Before treatment		34	33	33	Total	..	..	..
					Improved	..	..	..
After treatment		100	..	..	Unimproved	..	100	..

OXYGEN CONTENT.

Before treatment		..	100	..	Total	..	..	..
					Improved	..	..	..
After treatment		..	100	..	Unimproved	100	..	..

OXYGEN UNSATURATION.

Before treatment		..	..	100	Total	..	..	..
					Improved	..	..	..
After treatment		..	..	100	Unimproved	..	100	..

CO_2 CONTENT.

Before treatment		67	..	33	Total	..	..	..
					Improved	..	..	..
After treatment		100	..	..	Unimproved	50	50	..

average age in this group of patients is 36 years, the average age of the improved cases is 32, and of the unimproved cases 37 years. The average duration of illness was one year—in the improved cases it was about one-half a year and in the unimproved cases 1.3 years. The average duration of the treatment for all cases was 6 months, for the improved cases it was slightly above 4 months and for the unimproved cases 7 months. Of the improved cases one was a mixed manic, and the other an inadequacy depression. The former showed no variation from the usual picture of the mixed manic type of psychosis. Practically all the mixed manics and the other case of inadequacy depression are still in the institution at the time of this communication. In the face of these facts we feel that whatever changes in the laboratory findings occurred should not be ascribed entirely to the influence of the oxygen therapy. It is quite possible that the two improved cases would have shown improvement without the aid of the oxygen insufflations; and the changes in the clinical and laboratory findings which occur in the rest of the group depended upon factors other than oxygen administration.

Dementia Præcox.

Of the various groups of dementia præcox the catatonic form shows more evidence of organic manifestations, and especially cardiovascular involvement. We have selected for our study patients belonging to the catatonic group, having pronounced cyanosis of the extremities, with swelling and œdema. As catatonics are particularly negativistic we had to limit ourselves to 10 cases. None of the patients showed any appreciable improvement during the treatment.

1. *Cytology of the Blood.*—The hemoglobin was predominantly normal, with a few low findings both before and after therapy. There was no characteristic change coincident with treatment. The erythrocyte count was normal, with occasional high values, before treatment, and was normal after oxygen therapy.

Normal or high counts were observed for the white blood cells, with a definite tendency toward aggravation of the leucocytosis. In the differential count, the polymorphonuclear were found to be normal in the majority of cases and decreased on treatment, but the resulting variation was not outside the normal limits. The

lymphocytes were normal and remained so, showing no characteristic changes. This is also true of the endothelial cells, basophiles and eosinophiles, the latter 2 groups, however, showing a tendency to increase. Thrombocyte counts were made in only 2 instances prior to a course of treatment. In one case it was found within normal limits. In the other rather high. After treatment counts were made in all instances and were found normal or low. In both cases in which initial determinations were made, decreases were observed.

2. *Autonomic Signs.*—The basal metabolic rates in this group of catatonics were, on the whole, normal. No changes, except a greater tendency toward the negative end of the scale, were observed. It is of interest to mention here the case of a relatively young catatonic who, on several preliminary examinations, gave extremely high basal metabolic rates, the highest being 94 per cent above the average normal. This patient also had a pulse rate of 120 in the preliminary examination. Her eyes were slightly protruding, there was a slight fine tremor in the fingers, the color of her skin was rosy but there was no evidence of an enlarged thyroid. It is, however, quite possible that this patient had Graves' disease. It is of importance to note that during the course of the treatment the patient's basal metabolism decreased, at one time as low as 15 per cent and her pulse rate went down to 82. The blood pressure which was initially 154/90 dropped to 98/60. When the therapy was discontinued the patient's basal metabolism again rose and with it the pulse rate and blood pressure.

The blood pressures of this group lay mostly within the limits of 101 and 120, with 25 per cent of the cases over 150. A decrease was noted after treatment especially affecting those cases with initial high blood pressures. The pulse rates were mostly in the 71 to 100 range and dropped during the treatment. A very slight increase in weight is observed in this group of patients, but there are no marked changes.

3. *Changes in Blood Gases.*—The gas analysis was done in 8 out of the 10 cases and in all but one there was a post-therapeutic check-up. The oxygen capacity was usually normal, with some low and high values. No particular changes were observed during treatment. This agrees well with the hemoglobin findings. The majority of the cases gave low findings for the oxygen content, the rest being nor-

mal. A general tendency toward an increase was noted during treatment, although it was not great enough to change the distribution of the results. High values for oxygen unsaturation were the rule, with general decrease on treatment, changing the distribution so that more cases were within the normal limits. These cases cannot be correlated with changes in the basal metabolism or with changes in the hemoglobin or oxygen capacity of the blood. It must be concluded that improvement of the capillary circulation through the tissues of these catatonics, which is further demonstrated by the clinical amelioration of the cyanosis, is responsible for the figures obtained in the gas analysis.

The analysis of the venous blood for CO_2 gave normal findings initially, with a subsequent tendency to increase, although the changes were very slight and, as a rule, not outside the normal variations for a single individual.

REMARKS.

The cases of this group were the typical catatonics with pronounced negativism and resistiveness. They were practically all mute and most of them incontinent. In addition, they all had pronounced cyanosis of the lower extremities and a few of them ulceration of the skin of the legs. These physical manifestations usually disappeared in 2 or 3 months of continuous treatment. This is further evidenced by the increase in oxygen content of the blood, as we have noted above. Mentally there was no change whatsoever. All the patients remained just as negativistic as they were before. That the oxygen has some effect on these patients may be further presumed from the decrease in metabolic rate in the case with the possible Grave's disease, the general decrease in oxygen unsaturation, and the fall in blood pressure and pulse rate. The ages of the patients range from 27 to 50, with the majority in the early thirties. The duration of illness was from 5 to 16 years, with an average of 12 years. The duration of treatment was from 5 to 11 months, with an average of 8 months.

EPILEPSY.

The patients of this group belong to the usual cryptogenic variety of convulsive states. Their ages vary from 19 to 40. The onset of the convulsive attacks was in the first or second decade of life.

DEMENTIA PRÆCOX.

10 Cases—No Improvement.

HEMOGLOBIN.

Condition.	Per cent of cases. Normal.	Low.	High.	Condition after treatment.	Per cent of cases. Increased.	Decreased.	Unchanged.
Before treatment	70	30	..	Total	..	..	..
				Improved	..	..	..
After treatment	90	10	..	Unimproved	30	40	30

RED BLOOD CELLS.

Before treatment	80	..	20	Total	..	..	..
				Improved	..	..	..
After treatment	100	..	..	Unimproved	50	40	10

WHITE BLOOD CELLS.

Before treatment	50	..	50	Total	..	..	..
				Improved	..	..	..
After treatment	30	..	70	Unimproved	70	30	..

POLYMORPHONUCLEARS.

Before treatment	60	20	20	Total	..	..	..
				Improved	..	..	..
After treatment	80	10	10	Unimproved	30	70	..

DEMENTIA PRÆCOX.—CONTINUED.

LYMPHOCYTES.

Condition.	Per cent of cases. Normal.	Low.	High.	Condition after treatment.	Per cent of cases. Increased.	Decreased.	Unchanged.
Before treatment	70	10	20	Total	..	..	..
				Improved	..	..	..
After treatment	70	10	20	Unimproved	40	50	10

BASOPHILES.

Before treatment	100	..	..	Total	..	..	..
				Improved	..	..	..
After treatment	100	..	..	Unimproved	40	10	50

ENDOTHELIALS.

Before treatment	100	..	..	Total	..	..	..
				Improved	..	..	..
After treatment	100	..	..	Unimproved	40	50	10

EOSINOPHILES.

Before treatment	90	..	10	Total	..	..	..
				Improved	..	..	..
After treatment	80	..	20	Unimproved	70	10	20

THROMBOCYTES.

Before treatment	50	..	50	Total	..	..	..
				Improved	..	..	..
After treatment	60	40	..	Unimproved	..	100	..

DEMENTIA PRÆCOX.—CONTINUED.

BASAL METABOLISM.

Condition.	Per cent of cases. Normal.	Low.	High.
Before treatment	83	17	..
After treatment	83	..	17

Condition after treatment.	Per cent of cases. Increased.	Decreased.	Unchanged.
Total	..	..	..
Improved	..	..	..
Unimproved	33	50	17

	Toward normal.	Away from normal.	Normal before and after.
Total	..	..	..
Improved	..	..	..
Unimproved	17	16	67

BLOOD PRESSURE.

	100 and less.	101-120	121-150	151 and more.
Before treatment	13	50	12	25
After treatment	25	50	25	..

Total	..	..	..
Improved	..	..	..
Unimproved	37	63	..

PULSE RATE.

	70 and less.	71-100	101 and more.
Before treatment	13	67	12
After treatment	17	83	..

Total	..	..	..
Improved	..	..	..
Unimproved	33	67	..

DEMENTIA PRÆCOX.—Concluded.

WEIGHT.

Condition.	Per cent of cases. Normal. 100 and less.	Low. 101-125	High. 126-150	Over 150.	Condition after treatment.	Per cent of cases. Increased.	Decreased.	Unchanged.
Before treatment ..	20	60	20	..	Total	..	..	..
					Improved	..	..	..
After treatment ...	10	70	20	..	Unimproved	40	30	30

OXYGEN CAPACITY.

Before treatment	87	13	..	Total	..	..	..
				Improved	..	..	..
After treatment	58	28	14	Unimproved	43	43	14

OXYGEN CONTENT.

Before treatment	37	63	..	Total	..	..	..
				Improved	..	..	..
After treatment	37	63	..	Unimproved	86	14	..

OXYGEN UNSATURATION.

Before treatment	37	..	63	Total	..	..	..
				Improved	..	..	..
After treatment	57	..	43	Unimproved	..	100	..

CO_2 CONTENT.

Before treatment	100	..	..	Total	..	..	..
				Improved	..	..	..
After treatment	71	..	29	Unimproved	71	29	..

The patients showed only traces of mental deterioration, if any. They were brought to the hospital on account of episodes of confusion. During the course of treatment there was practically no change in the frequency of convulsions although there was an improvement in the general physical condition and they became more stable in their emotional reactions. It was, however, necessary after cessation of treatment, which lasted from 3 to 7 months, to treat the patients with phenobarbital or bromide in order to control the number of convulsions.

1. *Cytology of the Blood.*—The hemoglobin as well as the erythrocyte count was normal in most cases, with a few low readings. There was a definite increase after treatment. The leucocyte count was normal or high with no appreciable changes. In the differential count the polymorphonuclear cells were found to be normal or high, with some decrease after treatment. The lymphocytes were normal initially in the majority of cases, and showed slight increases, insufficient to change the distribution. All of the determinations on the basophiles, endothelial cells and eosinophiles were normal throughout the period of observation, although there were slight increases in many instances. In only one case was a post-therapeutic check-up made of the thrombocyte count. We may sum up the hematological picture by saying that during the period of oxygen therapy there was an increase in hemoglobin and erythrocytes, but no other marked changes.

2. *Autonomic Signs.*—The figures for basal metabolic rates were approximately normal although there were considerable percentages outside the normal range, values as high as 25 per cent and as low as 32 per cent being found. After treatment there was a tendency toward the positive end of the scale which produced rates still further from the normal as often as it increased the rate toward the normal.

The blood pressures were, as a rule, normal in this group, lying between 101 and 120. Changes observed were in both directions, but the decreases were more frequent than the increases. Pulse rates varied between 71 and 100. Treatment produced a definite decrease. Initially the weights of these patients were low or normal, but under the influence of treatment there was a definite increase.

3. *Changes in Blood Gases.*—The analysis of the venous blood was made in all cases both before and at the termination of treat-

ment. The oxygen capacity was normal or low at both times and, although there were absolute increases in the figures, they were not enough to change the distribution results. This is in accordance with the hemoglobin results which were also normal or low at first, tending toward improvement during the period of therapy. The oxygen content was low in the majority of cases, with little subsequent change. The oxygen saturation was high in 57 per cent of the cases, with an increase, if anything, at the end of the period. These gas changes are all very small and show no consistent relation between the direction of change of capacity and content. The CO_2 content was normal in most cases, high in a few, with no actual changes. There was a tendency toward an increase but the changes were negligible in amount.

Summary.

A comparative study of the findings in all groups cannot be presented accurately due to the lack of uniformity in the number of patients in each group. The initial findings for all groups showed a definitely normal hemoglobin content in all but a few cases which were low. There was no significant or general change after the course of oxygen therapy. This was correlated with a normal or low oxygen content, very few high values being observed. Both readings were in general slightly increased, if there was any change at all, in the involution melancholia, epilepsy and dementia præcox. The manics showed a slight decrease in both.

No striking variations were observed in the erythrocyte counts, which were usually normal before therapy, except perhaps some slight tendency of the few abnormal counts to come within the normal limits. In the majority of cases the leucocyte count was high before oxygen, and treatment seemed to aggravate the condition except, perhaps, in the cases which improved. These showed little change as a rule. Abnormally high counts were not observed as frequently after treatment, however, as before. All groups had normal or low readings for polymorphonuclear counts, no high counts being observed except in dementia præcox. A drop in the counts took place in the latter group and in the convulsive cases, whereas the other 2 groups showed no changes. The lymphocytes were normal throughout with a lack of low readings in the

EPILEPSY.

7 Cases—None Improved.

HEMOGLOBIN.

Condition.	Per cent of cases. Normal.	Low.	High.	Condition after treatment.	Per cent of cases. Increased.	Decreased.	Unchanged.
Before treatment	71	29	..	Total	..	..	..
				Improved	..	..	..
After treatment	83	17	..	Unimproved	57	28	15

RED BLOOD CELLS.

Before treatment	71	29	..	Total	..	..	..
				Improved	..	..	..
After treatment	100	..	..	Unimproved	71	29	..

WHITE BLOOD CELLS.

Before treatment	57	..	43	Total	..	..	..
				Improved	..	..	..
After treatment	71	..	29	Unimproved	57	43	..

POLYMORPHONUCLEARS.

Before treatment	86	..	14	Total	..	..	..
				Improved	..	..	..
After treatment	72	14	14	Unimproved	14	86	..

EPILEPSY.—CONTINUED.

LYMPHOCYTES.

Condition.	Per cent of cases. Normal.	Low.	High.	Condition after treatment.	Per cent of cases. Increased.	Decreased.	Unchanged.
Before treatment	72	14	14	Total	..	..	..
				Improved	..	..	..
After treatment	72	14	14	Unimproved	71	29	..

BASOPHILES.

Before treatment	100	..	..	Total	..	..	..
				Improved	..	..	..
After treatment	100	..	..	Unimproved	50	..	50

ENDOTHELIALS.

Before treatment	100	..	..	Total	..	..	..
				Improved	..	..	..
After treatment	100	..	..	Unimproved	57	43	..

EOSINOPHILES.

Before treatment	100	..	..	Total	..	..	..
				Improved	..	..	..
After treatment	100	..	..	Unimproved	43	29	28

THROMBOCYTES.
(Only 1 case compared before and after.)

Before treatment	100	..	..	Total	..	..	..
				Improved	..	..	..
After treatment	83	17	..	Unimproved	..	100	..

EPILEPSY.—CONTINUED.

BASAL METABOLISM.

Condition.	Per cent of cases. Normal.	Low.	High.
Before treatment	57	29	14
After treatment	43	29	28

Condition after treatment.	Per cent of cases. Increased.	Decreased.	Unchanged.
Total	..	..	..
Improved	..	..	..
Unimproved	71	29	..

	Toward normal.	Away from normal.	Normal before and after.
Total	..	..	..
Improved	..	..	..
Unimproved	29	28	43

BLOOD PRESSURE.

	100 and less.	101-120	121-150	151 and more.
Before treatment	14	58	28	..
After treatment	14	86	..	..

Total	..	..	..
Improved	..	..	..
Unimproved	43	43	14

PULSE RATE.

	70 and less.	71-100	101 and more.
Before treatment	17	83	..
After treatment	33	67	..

Total	..	..	..
Improved	..	..	..
Unimproved	33	67	..

EPILEPSY.—CONCLUDED.

WEIGHT.

Condition.	Per cent of cases.			Condition after treatment.	Per cent of cases.		
	Normal.	Low.	High.		Increased.	Decreased.	Unchanged.
	100 and less.	101-125	126-150	Over 150.			
Before treatment ..	43	43	..	14	Total	..	..	..
					Improved	..	..	..
After treatment ...	14	57	29	..	Unimproved	57	14	29
OXYGEN CAPACITY.								
Before treatment	57	43	..		Total	..	..	..
					Improved	..	..	..
After treatment	57	43	..		Unimproved	71	29	..
OXYGEN CONTENT.								
Before treatment	29	71	..		Total	..	..	..
					Improved	..	..	..
After treatment	43	57	..		Unimproved	57	43	..
OXYGEN UNSATURATION.								
Before treatment	43	..	57		Total	..	..	..
					Improved	..	..	..
After treatment	29	..	71		Unimproved	57	43	..
CO_2 CONTENT.								
Before treatment	71	..	29		Total	..	..	..
					Improved	..	..	..
After treatment	71	..	29		Unimproved	71	29	..

dementia and epileptic groups. No significant changes took place. All cases had normal basophiles, endothelial cells, and eosinophile counts throughout the observation period, except for a small percentage of high values in the eosinophiles of the dementia præcox group. There were no changes great enough to take the readings out of the normal range, although the tendency of the basophiles and eosinophiles was to increase, while the endothelial cells decreased. Thrombocytes were normal in both cases, with some low readings in the dementia præcox and melancholia cases. There was nothing characteristic about the blood pressure except perhaps a tendency to a slight fall. The pulse rate decreased in all cases, except for the manic group in which some increase was observed. The nutritional state of all the patients was low and treatment appeared to be coincident with improvement in some instances. In the majority of cases the basal metabolic rates were normal. Low values were also found, and high values in the groups of epileptics and melancholias. In these 2 groups there was a tendency to increase, but the variation in the other 2 groups were in both directions. The fluctuations were neither toward improvement nor aggravation of the abnormal cases, except in the one case of possible Graves' disease. The oxygen content was low in most cases, no high values were noted. In all groups except the melancholias there was some improvement of this condition on treatment. This was not correlated with improvement in the basal metabolism. Consistent with these results is the high CO_2 unsaturation found in more than half of the patients in each group. Oxygen therapy produced no consistent results, except perhaps that the improvement in the groups of dementia præcox and manics was more pronounced than the increase in the other two groups. The improvement in the catatonic patients may be ascribed to the improved capillary circulation, evidenced by the decrease in the cyanosis of the extremities. As a rule, the CO_2 content of these cases was normal, with a high percentage of high readings. This is consistent with the prevalence of low oxygen contents. In the catatonic and epileptic groups, a general increase was observed, but only in the catatonic group was it significant enough to exceed the normal limits in any instance. We shall not discuss further the rather meager results in the mental changes as they have already been indicated in the summary of each group.

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DISCUSSION.

DR. KARL M. BOWMAN (Boston, Mass.).—It is not very easy to discuss a paper when the first reader tells you that all the improvement that occurred came about naturally and was not due to treatment, and when the second reader points out all the possible flaws.

But, there are a few points that I think might be worth emphasizing. First, the fact that there have been in the literature statements that subcutaneous injections of oxygen produced improvement in cases of mental disease. Most of these articles have appeared in the French literature. The work reported was undertaken to confirm or disprove this hypothesis.

The authors say that they see no evidence of real clinical improvement which they would not have anticipated without such treatment, so that I think the clinical findings can be dismissed at that level. There are, however, two or three points with regard to the whole idea of oxygen therapy which seem to me to be worth emphasizing. For a number of years we have been hearing papers on the question of anoxemia and the idea that lack of oxygen to the brain is the possible explanation of many mental disorders. I think it was about 15 years ago that Mott brought forth the idea that dementia præcox was due to deficient oxidation in the brain.

As a result there has been a good deal of study along these lines and various attempts have been made to treat these mental disorders by trying to increase the oxygenation. It seems to me that theoretically it is obvious that subcutaneous injections of oxygen will not produce any increased supply of oxygen to the brain. Just how it could alter the rate or nature of metabolism or oxidative processes of the brain is quite vague.

In the first place, if you inject oxygen under the skin it will be absorbed gradually into the venous system. From there it will be carried to the heart and then to the lungs. This will simply mean that if you put any appreciable amount of oxygen under the skin there may be slightly less oxygen absorbed from the lungs by the arterial blood going out from the lungs, and that the amount of oxygen going to the brain cannot possibly be affected.

In the second place, we know that the arterial blood is 95 per cent saturated with oxygen anyway and that you have very little chance of increasing materially the amount of oxygen in arterial blood. The only way to do that

is to increase the number of red blood cells or the amount of hemoglobin so that the oxygen capacity of the blood will be increased or else to increase the pressure of the oxygen to which the blood is exposed so that there will be more free oxygen dissolved in the blood. It would seem that theoretically a method of treatment of this sort would not produce any increase in the oxygen supply to the brain.

The question of oxygen unsaturation was brought up. It is worth while pointing out that the findings from which the authors draw conclusions as to oxygen unsaturation were made almost entirely of venous blood. In only two cases, I believe, was the arterial blood examined as well.

I wonder—and I would like to ask that question—whether one is really entitled to take the findings in the venous blood, assume that there is a normal saturation in the arterial blood and therefore obtain the oxygen unsaturation by deducting from a supposed norm, or whether it is not possible that the oxygen capacity and the oxygen saturation in the arterial blood in certain types of mental disease may be abnormal. It seems to me that an assumption has been made here which may or may not be true, and that it is of considerable importance to be certain about this point.

I would like to emphasize that at the present time we do not know what the causes of certain diseases may be. For this reason we try out various methods of treatment and see what happens. Such attempts are quite worth while even though the results obtained may be largely negative.

In my opinion this paper represents an attempt of this sort and gives us certain definite data as to what happens under a specific method of treatment.

DR. F. H. PIKE.—There is just one point which comes out experimentally, that is that we can seldom hope to change one and only one condition in the organism at one time. In some experiments a few years ago in tying off permanently two vertebral arteries and one carotid in animals, we were able to tie off the other carotid at will and reopen it. When the remaining carotid was closed there was an immediate increase in the rate and amplitude, and there was an increase in the minute volume of respiration really surprising to one who has not seen it. At that time there was an increase in the oxygen in the arterial blood and a decrease in the carbon dioxide of the venous blood. That didn't result from any pathological condition in the animal; it resulted simply from a change, as far as we can refer it to one condition only, in the volume of blood flowing through the medulla in unit time.

In view of the results in cases of decompensating vulvalar lesions of the heart, it doesn't seem worth while to look for some mysterious change in the blood when, on purely physical grounds, the change in the volume of blood flowing through the medulla in unit time might equally well account for the high oxygen content and the low CO_2 content in these cardiac cases. If they happen to occur in mental cases, I doubt if the changed condition of the oxygen is due to the mental condition.

DR. J. NOTKIN (New York City).—Dr. Pike was unable in the limited space of time to go into details and I would, therefore, like to make a few

additional remarks. Of 13 improved cases of involution melancholia, 11 seem to belong to the so-called benign and two to the malignant group. Of 17 unimproved, two died and the rest are still in the hospital and all except three are of the malignant type.

In the dementia præcox group we selected catatonics only with pronounced cyanosis of the extremities. Not a single patient of the group improved mentally. The cyanosis, however, disappeared in every case. We also had a group of epileptic patients, in order to see what effect oxygen might have on the frequency of the convulsive attacks. Here again I must state that there was no change of any significance observed.

Finally, in a group of manic depressive psychosis, one mixed manic and one inadequacy depression, with a rather long duration of the psychosis, have sufficiently improved to leave the hospital.

On the whole, our limited experience with oxygen does not, as yet, warrant definite conclusions. We think, however, that the laboratory findings disclose some significant points. In addition, we do feel that the results of our study emphasize the value of the Hoch and MacCurdy classification of involution melancholia cases.

SOCIABILITY OF ABNORMAL CHILDREN AND SOCIAL CHILD PSYCHOLOGY.

(Sociability as a Mental Test of Child Groups.)

INVESTIGATIONS AT THE CHILDREN'S HOSPITAL, RANDALL'S ISLAND, N. Y.

(Preliminary Record.)

By F. SCHNEERSOHN, M. D.

I.

The Significance of Sociability and the Three Different Forms of Child Collectives.

Since 1921, we have systematically investigated in various countries the play and group life of normal and nervous children. In the course of these investigations there became more and more evident the presence of a child's world, little known, in which reigns a peculiar atmosphere of emotional manifestations, relations and tensions, that have their own regularity. In 1931, however, we had the opportunity to investigate the play and group life of mentally subnormal children.* We shall confine our preliminary survey to these latter investigations, because they are very characteristic and provide an excellent opportunity for comparing the group life of both mentally retarded and normal children.

The group life of children or the influence of group life upon the individual child is still relatively little known, although some investigators have called attention long ago to some aspects of this influence. In 1904 Meyer (Germany)[1] made a comparative investigation of the individual work and the group work of the child, *i. e.*, the work of a child when alone or when in a group. His conclusion was that group work is superior to individual work; group work stimulates the intellectual abilities of the individual child. To this

*I hereby wish to express my great appreciation to Dr. McGaffin, Superintendent of the Children's Hospital at Randall's Island, New York, whose generous aid enabled me to conduct my investigations.

very same conclusion came Schmidt [3] in his comparative investigations of the home work and the class work of the child. Of late Mede (Berlin)[3] and Bechterev (Leningrad)[4] conducted new experimental investigations (among children and adults) regarding the effect of the group on the individual. These and similar investigations [5] have proven more precisely the effect of the group upon the various abilities and reactions of the individual.

All these investigations, however, deal chiefly with the effect of the group on the *intellectual* life of the child. The effect of the group on the *emotional* life and on the intimate personality of the child has been studied far less. But, above all, it must be said that practically nothing has been done to study group life of children as such, *i. e.*, the structure of the group and the process of co-existence and co-operation of the whole group. The effect of the group upon the individual, however, depends upon how the group is built and how it functions.

We have elsewhere [6] shown that each group has its own structure which determines (according to its situation and interests) the rôle of the individual member and his ability to influence or to be influenced. In a play group of older children, for instance, a younger and weaker child would feel discouraged and hindered, while the same child in a group of its equals would be encouraged and stimulated. Even the same group will, under different conditions, have a varying influence on the individual, according to his character and position in this group.

First in 1921, through our clinical practice, were we induced to investigate the group life of children.* In our psychiatric child clinic, during our investigations of nervous children, we have often come across peculiar contradictions in the statements of parents and teachers regarding the conduct of the same child. In some cases, the parents would complain that the child was very nervous and restless, while the teacher would fail to notice any abnormality in

* It was at that time that I founded in Kiev the first Seminary for Social Child Psychology, which was later (from 1922-1927) continued in Berlin, and from 1928 on in New York.

A few years later, several German scientists, Schroeder [7] and Reininger,[8] started such investigations in other ways and by other methods. In the writings of Baldwin [9] are also to be found some indications of such investigations.

the behavior of the same child in class; whereas in other cases the teacher would complain of nervous agitation in a child in the class, while the parents would fail to notice anything abnormal in the behavior of the same child at home. In such instances, one is inclined to believe that either the parents or the teachers exaggerate the facts. But after a more thorough investigation one often finds a legitimate difference in the behavior of a child at school and at home. Insofar as the child succeeds or does not succeed in finding its proper place in the child collective, can the influence of the collective have a stimulating, a quieting, or a discouraging and denervating effect on the child. Just as the investigations of Schmidt (mentioned previously) find a difference between the home work and the school work of children, so we can also find a difference between the home behavior and the school behavior of the child, according to the positive or negative influence of the child collective. But in time, the influence of the collective can produce a deep and lasting effect on the general life of the child.

The child alone, and the very same child in a group—differ widely. We must differentiate in children between individual and social psychology, just as we do in the case of adults. In other words, we must learn to know the psychology of child collectives. Pedagogues generally deal not with individual children, but with child collectives—school classes, kindergartens, etc. Pedagogy must, therefore, be based not only on individual, but on social child psychology. It is important to know the psychology of the entire collective which we are to influence, as well as the sociability of the individual child, *i. e.,* the grade and the character of the child's ability to influence and to be influenced in the child collective. It has already been ascertained in various fields of child study that the child is not a miniature of the adult, but has its own peculiar nature, and so the group life of children is not a miniature of the group life of adults, but has its own sphere of life and development. We can thus briefly formulate *social child psychology as a science which concerns itself with the mental group life of children in their concrete entity and peculiarity.*

Already at the start of our investigation we came across interesting evidence of three different forms of child collectives: There is, namely, the family collective, the local collective, and the school collective. Each of these has a different structure and also a different significance in the development of the child.

The family collective consists of a child's brothers and sisters whose ages are still close enough to enable them to form a play group. The characteristic qualities of such a collective follow: First, in a family group the children have common hereditary qualities and blood relationship, which make the relations of the group organically intimate. Out of the family group the child carries experience, memories and warm attachments, which last throughout its life. An only child is much the poorer in this respect. Secondly, in the family group the children share the common home life, the innumerable interests of the family milieu. They share, for instance, the attention of the parents, the place at table, the nursery, toys, etc. The child learns here from its earliest years that he is not the only one in the world; that he must share almost everything with his intimates; he must get along with them. If he snatches anything from his brothers, they will retaliate, and how! Every fight here is hard and instructive. In the family group the child receives the first and decisive education in self-restraint, discipline, duty and co-ordination. The only child has not such opportunity for self-development. If it is not given a compensating social education, then in its adult years it shows disturbing symptoms of insufficient self-restraint and unsociability.*

The local collective is formed by children of a neighborhood children from one building, one yard or one street. Here the children are united by local proximity; they share together the many-sided interests of the common local milieu. A garden, a few trees in the yard, a hiding place around the house, a wall to climb, a familiar cat or dog in the yard—all these and innumerable other local interests unite the children. Investigations have shown that the local contacts between neighborhood children can be very intensive and intimate. Whereas, the place for the family collective is the intimate home milieu, the place of the local collective is the outdoors—yard and street—which, through open and hidden charms of nature awaken in the children deep instincts and emotions. In leisure moments, at dusk, out of school and out of the

* Dr. Neter[10] (in his article "The Only Child and Its Education") describes the characteristic qualities of the adult who was an only child. In a public letter he asked for their reactions to his article and for a description of their personal experiences. The replies received were very characteristic. By comparative investigation of child fantasy,[11] we could establish the respective characteristic qualities in the emotional life of the only child.

house, in hidden corners, under the free sky—the children display the most daring, often secret games, in which they live through the deepest emotions and instincts. Such games, therefore, have a special significance for the investigations of child group life.

The school collective is formed by children of the same school, who share the fate and the many-sided interests of school life. Here we have a collective of higher order. The formal system of the school, with its divisions according to age and ability of children, brings with it a stronger sense for organization in the school collective. Common instruction and specific school atmosphere intensify the spiritual nature of the school collective. The school collective can unite children over a period of years, and such a collective, therefore, often has its own history. The school, to a large extent, takes in the life of the child; determines often the fate and the future of the child. The common school interests are, therefore, fate-interests, and the school collective is thus a fate-collective.

The social child psychology deals with all the above three forms of child collectives. Its chief object, however, must be the school collective, which has history and is the most organized, the most stable and many-sided fate-collective. There are also mixed forms of child collectives. Homes for children are at the same time a local and a school collective. To this type belongs also the Children's Hospital at Randall's Island, where we studied the group life of mentally retarded children.

The main condition for such an investigation is observing the child collective when left on its own and the children have relatively unlimited freedom of play and movement. The school collective, for instance, should be observed during recess and not only in time of instruction when the children are under the influence of the teacher, study program and discipline. At Randall's Island we made an attempt to observe the children in moments of relative free play when the children could give freer reign to their natural inclinations.

The methods of investigation are as follows: (1) The method of general observations, when we observe and describe all manifestations of group life (see next chapter on principles of such observations)—the conduct of the children and their mutual relationships, as well as their relationship to the entire collective. (2) The special play method, when we concentrate our observation on the

children's group games, which in their way reflect (see Chapter VI) the collective emotional relationship of the participants in the play. (3) The pathological method, *i. e.*, the comparative investigation of the group life in normal and abnormal children; in the case of the latter we have group life in unusual or abnormal conditions; it is certain nature-experiment which gives a new approach to group life.

II.

SOCIABILITY AS A MENTAL TEST OF CHILD COLLECTIVES.

(Investigation Results.)

In studying the sociability of mentally retarded children, we ask ourselves the following questions; Whether and to what extent such children are able to have contact with each other, to have and share common interests, to influence and to be influenced in the collective? In other words, what grade of intelligence is necessary for one or another stage of sociability?

In our studies of mentally retarded children it proved helpful to use as a yardstick the two basic principles which we established in the collective life of normal children. The first basic principle is the so-called " group principle," as follows: When we, for instance, observe the normal school class during recess, we always notice that the children do not play together, but break up into groups. Children of equal or similar character congregate into a group with common interests—each group has its own structure and its own games. In each group the active and passive elements come to light, especially the leader. Common instruction and school discipline unite a large number of children in the class collective, but only in the smaller group can similar characters find their natural expression. During recess, therefore, the collective is divided into play groups. Outside the groups there are usually a few " lonely " children who cannot fit themselves into any group—they are, therefore, lonely and have neither standing nor influence in the collective. The so-called " class spirit " is created chiefly by the groups, which are the active units in the collective, and not by the individual children. Outside of the groups, the individual children are " lonely," or socially ineffective. As with the adults, parties break up into fractions and sections, so with children—the collective breaks up into groups. The relations between the children in the group, and

those between the groups in the collective, make up the complex structure of the collective. The number, the organization, the stability of the groups, as well as the number of "lonely" children—are characteristic symptoms of the sociability of the collective and its members. The group in principle, therefore, indicates whether and to what extent the individuals can co-ordinate themselves in related groups and to what extent the groups can co-exist, in the collective.

The second basic principle is the play principle. The free activity of the child is, for the most part, a play activity which expresses the interests of the child in two ways: Firstly, the play activity is spontaneous and reflects the vivacious *emotions* of the child, its *emotional* personality. Besides, the play requires the co-operation of the creative fantasy, with the help of which only, can the child fancy itself and its playmates in various parts and imaginary situations. The play activity, therefore, expresses the emotional creative personality of the child. Secondly, the play activity is, for the most part, collective, as mentioned above. Normal children seldom play individually, but mostly play with each other in a play-group. They play as a collective; emotional creative activity thus reflects the innermost connections within the play-group. Each group has its own games which characterize the dominating group tendencies. A girl's group, for instance, frequently plays "Papa-Mama," rhythmical round dances, etc., while boys often amuse themselves by playing fighting games which express their dominating adventure-fight instincts. In every group-game, however, each child has its own part which enables it to express its individuality in the play-group. The play-principle thus indicates a new possibility for peeping into the inner collective life of children.

Let us illustrate this with the typical picture of a normal class collective during recess. The following is an abbreviated record of some of our methodical observations:

Public School, Warsaw, Dziki 6. Grade I; 23 children, ages between 6-7 years; first recess.

The collective breaks up during the recess into three groups. The first group—eight girls—play "Windmills"; this is a popular game in all schools and it is accompanied by a special song. In this game the children cross hands and move rhythmically in a circle, stamping their feet to the beat of the song. On the initiative of the girl Malkale a slight modification was introduced into the game; namely, with the tips of the fingers of their free

hand the girls were daintily to take hold of the hem of their skirt. Malkale is generally known in class for her ideas and suggestions. The second group of three boys play "Dogs." The children run after each other, barking, until they all successively fall to the ground; then they begin to bark loudly, to tease and to bite each other. These three boys always play together, and the leader is most always Nachman. Should he once be absent from school, the other two boys do not play; they are then rather passive observers of other play-groups.

The third group—four boys and eight girls—are engaged in organized school games under the supervision of olders.

The remaining four children are unadjusted. Two of them, girls, are "wanderers"—they move from group to group, they cannot fit in any one, and disturb the games all over. The other two girls are "lonely." One is always sad, whimpering and downcast; she never takes part in the games. She is an orphan, being brought up by a foster-mother in great poverty, and is frequently hungry. The other girl is very quiet, bashful, is very fond of her lessons, and during recess she remains in the classroom and writes on the blackboard.

The play principle is here clearly illustrated along with the group principle: Three fairly organized stable groups, in each the leader is prominent, and a few "lonely" children. Each group has its characteristic game; the rhythmic dance play of the tender girls; the "dog" play, with barking and biting, of the adventurous-pugnacious boys.

It is interesting to observe these very same two principles in the collective life of mentally retarded children, which we had occasion to do at Randall's Island, New York. Here we made our investigations by the two above mentioned (Chapter I) methods.

At Randall's Island the various categories of retarded children are so grouped, that the children of very low mentality, the idiots and imbeciles, are in one division, and the less retarded children of various categories—from high grade imbeciles to border-line children, with a large majority of morons, are in another. Out of the extensive investigation material we will merely give a characteristic record of each of the two mentioned divisions. The first record is from Nightingale Hall. (Idiots—29.4%, Imbeciles—70.6%.)*

Randall's Island, Nightingale Hall: Friday, April 24, 1931; 3:20 p. m. 81 female children; ages 7-10 years. Idiots.

* This classification is taken from the official records of the Department of Hospitals, Children's Hospital, Randall's Island, New York City. The mental examinations were made in the psychological laboratory of this institution, by Dr. Louise E. Poull.

The children are seated on chairs in two separate groups, each group forming a square along opposite walls of the room. One group is somewhat larger—having 45 children—the other, 36. There is a nurse in charge of each group. At our first glance we were struck by the isolation and separatism among the children. Each child was sitting for itself and had no relationship with the children sitting next to her. With few episodic exceptions there was no group formation among them. Some of the children were sitting immobile, rocking forward with legs folded or hanging down. Others made stereotyped movements. Some kept a finger in the mouth, legs dangling, arms moving, or, covering their eyes with the palm of their hand, were leaning their heads on their arms. Two children moved their bodies backward and forward, automatically; one child masturbated openly.

During the period of our observation it occurred that one child turned to her neighbor and touched her, another left her chair and went to a more distant neighbor, remained standing or touched the neighbor's hand. But all this was of momentary duration. Soon, they returned to their previous state of isolation and indifference, each child for itself, looking straight forward, unmindful of the other children.

Silence prevailed in the room, as well as among the children; only an occasional sound disturbed the general quietude.

The entrance of the observers made no particular impression on the children. Only two or three of those sitting right opposite us attempted to leave their chairs, but were restrained by their nurse. The other children looked at us occasionally, but showed no interest in us. One of the three children, who were disturbed by our entrance, a colored girl, followed the teacher in our group, held on to the teacher's skirts and displayed a strong attachment.

Here we see that in the case of the much retarded children at Nightingale Hall, we have a definite low stage of sociability, which is strikingly characterized by the isolation of the children. The children sit by each other, but not with each other. There is neither group formation, nor common playing outside of some traces of hints, of which we will speak in the next chapter. Some children play with themselves, with their own bodies (leg-swinging, hand-movements), some children make automatic movements. A striking symptom here is also the lack of that loud vocal-verbal communication between children which usually creates the characteristic tumult of a child collective. In short, the sociability of imbecile collectives is characterized by the following symptoms:

1. The lack of vocal-verbal communication (immobile silence).
2. The lack of group formation (isolation).
3. The lack of collective playing (playlessness).
4. Self-play, as well as automatic movements by some members of the collective.

Now we come to the record of the second division, in Sunnyside Hall (Randall's Island), where we have the mixed groups of less retarded children—from slightly imbecile to border-line children:

Randall's Island, Sunnyside Hall; Tuesday, April 28, 1931; 16 girls and 12 boys; ages 5-8 years.

Immediately on entering Sunnyside Hall, we were struck by the din and tumult of the children. Only three of the children were sitting lonely and inactive. All the others were continuously astir, shouting and running. There was neither plan nor goal in this activity; it was rather chaotic. No one tried to outdo the other in noise or movement. Voice and action mingled chaotically, and every object was episodically engulfed in the chaos.

While running, children came across an overturned wooden chair, which was pushed and pulled by them. None of them tried to monopolize the chair, none tried to take it away from the others. One minute two children pulled the chair, leaving it the next minute, others pulled or pushed the same chair, and a few minutes later all of them left the chair. The children were not active individually, all of them ran in smaller or larger groups. But the groups formed, dissolved, and reformed continuously. *There was no constancy in them and no organized play of any contents.* Only two boys had played, for some time, with a feather. They caught it, let it fly, blew it, caught it again, etc.

From this we see that the less retarded children in Sunnyside Hall show a definite higher stage of sociability, which is characterized by a considerably higher grade of contacts between the children. The children move in *groups,* stimulating and imitating each other—group formation! Common play movements dominate the entire collective, which continues to run, to jump and to shout for a prolonged period of time. Here, the ability for group formation and common play is indicated. But the groups are fluent, forming and dissolving continuously. They have no stability nor differentiated relations; the games are primitive, chaotic, without contents, structure and different parts, with the exception of some traces which we will discuss later. Very symptomatic is the characteristic noisiness, the verbal-vocal communication. However, the noisiness here is also chaotic, lacking those regulating shadings which we find in the normal group-organized child collective. The sociability of the debile collectives is, therefore, characterized by the following symptoms:

1. Chaotic noisiness (vocal-verbal communication),
2. Unorganized fluent groups,
3. Undifferentiated (structureless) playing.

The higher the intellectual level of the collective, the more organized will be the groups, the more structure and contents will there be in the games.

During a period of four months we investigated the above-mentioned, as well as other divisions at Randall's Island. The observations were made at various times and under various conditions—but always with the same results.

Following is a brief record of some investigations made under changed conditions, whereby we eliminated the influence of the educator and other evident limiting features. In this way, the children could show their natural tendencies more freely.

On several occasions we made the following changes:

We asked the attending nurses to allow the children freedom of action without supervision. They were taken off their chairs and benches, and permitted to move around at will. In both halls (Nightingale Hall and Sunnyside) the symptoms indicated above appeared even more striking than before. In Nightingale Hall most of the children remained standing immobile near each other, only in a few instances did a child let his hand stray momentarily to the shoulder of another child. One child with a piece of broken comb, touched the head of another; two children have for a moment mumbled something to each other.

But the quietude, isolation and separatism of all the children was rather emphasized, in view of their physical contact and proximity to each other.

While at Sunnyside Hall the din, noise and moving about of the children grew and was more chaotic, arriving at a point of untamed wildness; the groups became more fluent and momentary, almost disappearing. The running was hurried, losing all form.

So, under various conditions, each category of retarded children shows its characteristic symptoms of sociability. As the individual intelligence test was heretofore applied, so now sociability gives us a collective mental test, *i. e.*, an intelligence test of child collectives. In order to establish the intellectual level of a collective, we must leave the collective on its own and under conditions of the utmost possible freedom of play and movement—and then register certain behavior characteristic of the various degrees of mental retardedness, as described above—movements, communication, group formation, characteristic games. (Idiots have neither communication, nor group formation, nor common games. The groups, beginning with slight imbeciles up to borderline children, have communication, groups, and games, which are, however, primitive, chaotic and unorganized. The higher the intellectual level—the more organized

and differentiated are communication, group and games.) On the other hand, sociability is also a certain intelligence test for the individual child. A physically healthy child, under normal conditions, can be tested intellectually according to his sociability, *i. e.*, whether and to what extent it (the child) takes part in communication, groups and games.

We have, however, indicated above that sociability is a decisive factor in the emotional life and behavior of the child. We must, therefore, in treating nervous children, methodically define their sociability, which can be a cause or a condition of their nervousness.

III.

THE TREATMENT OF SOCIABILITY.

(Trace Principle.)

The investigations at Randall's Island proved that at every stage of limited sociability we can always find traces of a higher stage. The much retarded children at Nightingale Hall, for instance, who lack the ability for group formation and collective plays, however, always show traces of a higher sociability. On the general background of silence and isolation one invariably notices here and there single momentary reactions of sociability. This is evident in the above descriptions of our investigations. To recall briefly; one child turns its head toward its neighbor and puts its hand silently upon the latter's shoulder; two children sitting next to each other murmur monotonous syllables to each other; one child leaves its chair and, approaching the child opposite, remains silently at its side, touches its hand or plays with its shoe-lace; one child touches the head of another with part of a broken comb. Such and similar reactions repeat themselves now and then for brief moments, after which the children return to their usual state of silence and isolation.

Once, at Nightingale Hall, on April 28, we took note of an unexpected little incident. In the larger group, which was seated on chairs to the right of the door, several children "suddenly" left their chairs and congregated into a group. It was very striking and we sharply observed it. This sudden group lasted for a few minutes in a common play and dissolved shortly after. During this play, the older of the girls, eight years of age, held a string of beads in

her hands, while the other children were helping her string and unstring the beads, at the same time making stereotyped movements of the lips. " Suddenly " the children returned to their former state of silence and isolation.

In the last case we clearly see how the group play suddenly flared up for a few minutes and then immediately subsided. However, during this momentary flare-up we see not only traces, but significant evidences of higher sociability. In this " sudden " group the children are fairly connected and co-ordinated. The simple playing with the beads is somewhat regular and consistent; there is even a certain division of parts, with leader and helpers. Here we have an " outburst " of higher sociability, which breaks through for a while and then subsides.

The less retarded children at Sunnyside Hall, who have a limited ability for group formation and collective playing, in every level show traces of higher social ability. Amidst the chaos of fluent groups and dull play movements, there flares up repeatedly the group of two boys with their consistent feather game, which comes as an " outburst " of a more highly organized sociability.

The presence of " traces " or " outbursts " has practically a clinical-educational significance, because it proves that however limited the sociability may be, there are, nevertheless, always latent possibilities in the child which might under favorable conditions come to an " outburst." This is a guide for the special education, which should act in the direction indicated by the traces and accordingly develop the latent sociability of the child.

When dealing with much retarded children, we should, with the help of systematic grouping and suitable toys, methodically stimulate group formation and collective playing. The spontaneous, episodic outburst proves to be a guide for the grouping and also for the choosing of games and toys. When dealing with less retarded children, whose sociability is chaotic-primitive, we should awaken through appropriate organized games the latent ability for organized social tendencies. The organized games should not be introduced (as is frequently done) from " without," nor should there be one game for the entire collective: First, such games are foreign and artificial and they tend to flatten the already low sociability; second, such games are not differentiated for the various groups in the collective and they obliterate still more the chaotic sociability. Not one

game for all—but an appropriate game for each group. The methodic grouping must be the starting point for the organized playing and should develop the physiognomy and the organization of group and play from within. Here, also, does the episodic outburst serve as a guide for grouping and game choosing.

Normal children also have latent potentialities of a higher supernormal sociability which, under favorable conditions, comes to an "outburst" in enthusiastic heroism and moral inspiration. In observing such "outbursts" in children, we perceive and can methodically educate the latent social creativity of the child. For this purpose, we must also observe the child's natural group relations and group plays (See Chapter VI), which characterize in a concrete way the "outburst" of higher sociability.

As a matter of fact, when some years ago we made investigations of the fantasy and emotions of mentally retarded children, we here, also, uncovered [12] the evidences of "outbursts"—as described in detail elsewhere. This investigation showed, namely, that the limited imagination and emotionality of mentally retarded children have their latent possibilities which might frequently appear as an "outburst."

All this leads us to believe that not only the above-mentioned (sociability, imagination and emotionality), but most of the other limited abilities of retarded children also have latent possibilities which manifest themselves in "outbursts." The more we learn to recognize the character of these latent forces and their "outbursts," the more will we be able to use them in the education of retarded children.

To be sure, the "outburst" is universal in our mental life. The artist, for instance, is not always creative, but only in moments of inspiration does he reveal his utmost creativeness. It sometimes flares up so "suddenly," that the artist has the feeling of "outburst." Also, in our every-day life we often enjoy the "outburst" of long-sought ideas, which "suddenly" flare up in the mind. The artistic inspiration, as well as the sudden idea, however temporary they may be, leave traces and products of the latent possibilities revealed. So also with retarded children—we can educationally grasp the signs and products of those latent possibilities which manifest themselves unexpectedly in "outbursts."

IV.

Sociability and Play of Nervous Children.

As in the case of mentally retarded children, so we also observe the phenomenon of "loneliness" (asociability) in nervous children—however, in an essentially different form. We must differentiate, namely, between organic and functional loneliness.

Mentally retarded children organically lack sociability, according to their degree of retardedness. Thus these children are organically asocial or lonely. It was so striking to see the above-described picture at Nightingale Hall (Randall's Island), where eighty-odd mentally retarded children were sitting in one room, close to each other—yet all lonely, isolated, each one to himself. A collective of lonely individuals! Here the loneliness is organic, static, and elementary, inasmuch as the retarded children organically have neither sense nor urge for sociability.

Entirely different, however, is the loneliness of some nervous children, in whom sociability is merely checked by emotional disturbances. There are some such lonely children in every normal child collective, as shown in the above-described typical picture of a school class. This sort of loneliness in nervous children is functional, dynamic and reactive, inasmuch as nervous children organically have sense and stimulation, often even exaggerated, for sociability, these being only emotionally disturbed. The relation between loneliness and nervousness becomes clearly defined as soon as we are familiar with the causes and conditions which bring on loneliness in children. Following are the causes which we established in our investigations:

1. *Depression.*—Marked depression checks the sociability, as it does other active reactions, and brings on self-isolation and loneliness. This was the case with one of the lonely children in the above record of a normal school class.

2. *Abnormal Bashfulness.*—This can be the only or the most pronounced symptom of the child's nervousness. Because of bashfulness such a child cannot answer the teacher's questions and often makes the wrong impression of retardedness. This child is generally bashful with other children and is frightened by people on the whole. In this way it avoids its schoolmates and remains lonely in the child collective. The abnormal bashfulness seems to be inherited. At any rate, in every case which we treated, we established that one of the parents was suffering from abnormal bashfulness.

3. *Nervous Mobility.*—This we find in some children along with restlessness and distraction—all of which form the symptom complex of Demoor's "psychic chorea." Such children have difficulty in coordinating their reactions and relations in the group. They become aggressive and disturbing in group playing, other children avoid them, and they often remain lonely.

4. *One-Sided Abilities or Their One-Sided Development.*—This can make children become absorbed in their own inner world. Such mental one-sidedness is often found in nervous children; however, it is at times not the cause, but the effect of loneliness. Unable for various reasons to adjust itself to the collective, the child seeks to develop its own inner world and to become self-sufficient.

5. *Sociological Disturbances, i. e.,* social conditions which make the approach to the collective difficult for the child and thus, by force of circumstances, it becomes lonely. A poor, unkempt child, might feel humiliated and isolated in a collective of brightly dressed children of the "high-brow" type. This also holds true with regard to racial differences.* Each striking characteristic in the child can, under certain conditions, become a social disturbance.†

6. *Educational Disturbances, i. e.,* educational influences which are unfavorable to the normal development of the child's sociability; for instance, an only child whom the anxious mother isolates considerably from other children, or a child who for some reasons studies at home and, being outside of the school, has no contact with child collectives. The same is true of emigrant children who, because of differences of language and manners, cannot at first make contacts within the strange school collective, and they remain lonely in it. Children who frequently change school have not the opportunity to develop their relations with other children and are, therefore, lonely to a great extent.

In the last two cases, *i. e.,* in cases of sociological and educational disturbances, loneliness can be the cause of the nervous disturbance of the child's mental equilibrium. Also in the first cases, where loneliness first comes as a result of nervous disturbances (depression, bashfulness, restlessness, etc.), loneliness, once present, can intensify those nervous disturbances. In other words, the asociability is a pathogenic factor which can awaken or sharpen nervous disturbances in children and must, therefore, be considered as a

*We had a case at the Child Psychiatric Clinic in Berlin with a boy eight years old, who in his third school year suddenly, as it were, became restless, unruly and poor in his studies. Investigations showed that he was the only Jewish boy in the Christian class. The other boys, having become aware of his Jewish parentage, began to tease him and isolated him. The boy became depressed and discouraged.

† The following is a typical case; A boy seven years old is red-headed. The children in the class gave him the nickname "Fire." The boy is very embittered and helplessly lonely. He turns aggressive, restless and unruly.

very important point in the diagnosis and treatment of nervous children. It is, at the same time, an important task in mental hygiene for every teacher to observe whether and how many lonely children there are in the class. The teacher must in each case find out the cause of the child's loneliness and accordingly help the child to adjust itself gradually to the group games of the collective.

In observing more closely the pathogenic influence of loneliness, we get a clinical insight into the play problem. The lonely child, namely, remains outside of the collective play activity, which is the principal activity and main interest in the child collective. Thus, we have an opportunity to investigate the essence of play by observing the specific effects of its absence. The investigation of playlessness is as decisive a method as the pathological method in physiology.

Already in every-day educational life, one can observe the very characteristics of playlessness. When a normal child lacks the opportunity for suitable play amusement, it is likely to be in the painful state of ennui. The child does not know what to do with itself nor wherewith to interest itself; all seems gray and empty in its eyes. The feeling of emptiness is subjectively the most characteristic essence of ennui. If this state continues for too long a time, this emptiness can become unbearable.

V.

The Clinical Play Theory.

In observing more closely the pathogenic influence of loneliness, we get a clinical insight into the play problem. The lonely child remains outside of the collective play activity, which is the principal activity and main interest in the child collective. Thus we have an opportunity to investigate the essence of play by observing the specific effect of its absence. The investigation of playlessness is as decisive a method as the pathological method in physiology.

In the every-day educational life of children one can observe the very characteristics of playlessness. When a normal child lacks the opportunity for any play amusement it is likely to fall back into the painful state of ennui. The child does not know what to do with itself, nor wherewith to interest itself; everything around it seems "empty" and colorless. The feeling of emptiness is subjectively the most characteristic essence of ennui. Not only does the child

feel "empty" inwardly, but the outer world, too, appears "emptied" and faded. If this state continues for too long a time, it can become unbearable. The child seeks impulsively to fill this emptiness and instinctively uses all his possible dispositions for this purpose. The child becomes nervous (excitable, capricious, crying, restless), or the child compensates its emptiness by masturbation (compensatory masturbation), or it finds an outlet in spiteful antisocial passions.*

Nervousness and dissipation are thus defensive reactions whose aim it is to compensate for the emptiness of ennui or playlessness. The relations between play and nervousness is organic and dynamic—like the two plates of a scale they balance the equilibrium of mental life. When the scale of play descends, up rises the scale of nervousness or dissipation. In lonely children with their chronic playlessness we will, therefore, find a chronic state of nervousness and dissipation.

The clinical observations of the relations between play and nervousness lead us to the new biologic orientation which began in physiology with the theory of vitamins. It was discovered (about 1910) that the absence of vitamins leads to scurvy and other deficiency diseases, though these vitamins do not supply the body with energy of building material, but have only stimulating effects.

Principally and essentially, we are faced here with a new biological orientation which investigates and begins to recognize the vital importance of "only" stimulating factors.

This biologic orientation must also influence our views and treatment of neuropsychic life. The neuropsychic life, too, requires such factors which have little relation to the concrete mental needs of the person, but have chiefly a stimulating effect—"psychic vitamins."

The clinical play theory can, therefore, be expressed in the following way:

Just as the "useful" work satisfies the concrete needs of the struggle for existence, so the "useless" play is of vital importance, as it stimulates the urge to the life struggle and sustains the tonus of mental life. Lack of the play elements—"psychic vitamins"—leads to nervousness.

*We do not mean to imply that one urge is transformable into another, but the emptied personality uses one or the other autonomic source, depending upon character, education and situation of the child.

These clinical considerations (elaborated by me more fully elsewhere [13]), throw a new light on both the problem of play and that of nervousness; the play offers the vital psychic vitamins and the nervousness is, so say, a mental avitaminose or psychic scurvy.

Whatever our view on this clinical play theory may be, we must recognize its basic value in establishing the dynamic relationship between loneliness, playlessness and nervousness. It offers us, as described above, important points for the diagnosis and treatment of nervous children.

Further clinical observations bring a deeper understanding of play and its relation to nervousness. We must differentiate between productive and destructive play. The former awakens the potential intimate creative forces of the child and brings him thus to a higher level of development. Athletic, esthetic and other social games of children stimulate generally the child's fantasy and emotion with the all-dominating impetus of play. Moreover, each particular game gives rise to special emotions which are stimulated by it. Productive play we define as intimate, because it stimulates the inherent creativeness of the child and makes him more profound and inwardly richer. Quite opposite is destructive play which awakens the child's primitive, degrading potentialities and thus brings him to a lower level of development. Such, for example, are the anti-social amusements (the old "pyromania," "cleptomania," etc.), the masturbating play with the sexual organs, games of cruelty with animals and weaker playmates (the pulling off of the wings of flys, the torturing of small animals, the teasing, pinching and slapping of younger or weaker children). These destructive games we call primitive, inasmuch as they stimulate the primitive passions.

Between these two types of play there are various transition forms, as well as mixed forms. However, we must differentiate between the intimate and the primitive games which have different sources and effects on the child's mental life.*

* The recapitulation theory of Stanley Hall [14] and the relaxation theory of Patrick,[15] emphasize certain similarities between play of children and the ancient pursuits of primitive people. Hall asserts that the individual recapitulates in the play of childhood, the primitive stages of the race. Patrick, too, points out that the ever increasing strain of adjustment (work) in modern civilization makes necessary play through which the individual may relax "regressing" to the more elemental and simple old racial activities. Both

When the child lacks the opportunity for productive play, he impulsively seeks an outlet (differing in each individual according to character and situation), in various destructive games of primitive passions. In other words, if the child cannot live his life intimately (creatively) then he lives it primitively. If, however, the child is by nature too sensitive for primitive play, then there arises a nervous excitement which, with painful tension, may fill in the inner emptiness of playlessness. In this state, resulting from "emptiness," the child cannot control the constitutional weaknesses which develop into nervous symptoms. These symptoms carry, as play, but in a nervous primitive way, the fascination and tenseness of pain and excitement. The neurosis, or psychic scurvy is thus a primitive compulsive play which arises from the lack of intimate productive play.

Now, comparing objectively the psychology of work and play, we can bring the clinical theory to a conclusion which is also of practical significance. In work, expression is given to the awakened, *normal consciousness* which thinks rationally and acts *purposively* in accordance with the requirements and goal of the work. In play, expression is given to the effective or "spherical" consciousness which thinks intuitively and acts impulsively in accordance with the free passions of play. These passions can be intimate creative, or primitive destructive—but find their expression in the above described two kinds of play. These *three* categories of mental life—normal consciousness, intimate spherical life and primitive spherical life *—are prime sources and irreducible. They are organically united, and together build up the multifarious unity of the personality.

The normal consciousness is dominating in the struggle for existence where a rational, purposeful attitude is essential. But, no matter how dominating the normal consciousness may be, each person

theories emphasize in play, only the primitive, ancient elements. *They overlook the productive play and its various creative elements which lead the individual not "back" to the primitive past, but "forward" towards new achievements.* Theatricals, concerts, literary evenings, highly developed physical sports, festivities of modern world religions, etc.—are as far from primitive activities, as modern technique from the primitive tools of our ancestors.

* These find their expression in the life order in work, intimate and primitive play, which form the rhythm of human activity.

has, nevertheless, his own particular spherical or effective urge, differing in each individual, in grade and character. Psychic vitamins or play elements are, thus, such experiences as awaken and satisfy the individual spherical urge.

Work is always followed by a rest period, with its natural accompanying play diversion which expresses and satisfies the individual's spherical urge. Children, not needing to wage an independent struggle for existence, and having as yet, insufficient ability to discriminate between reality and fantasy, their normal consciousness being dominated, therefore, by the spherical, effective life, they play rather than work. Psychologically, therefore, the play urge in children is stronger and more unrestrained than in adults.

When the actual life order leaves unsatisfied the individual spherical urge in an intimate creative way, then the individual impulsively seeks an outlet in nervousness or primitive play wherein he may free himself at the first opportunity by an intimate burgeoning. Nervousness, or psychial scurvy, is thus dynamic expression of a spherical deficit which results from a chronic lack of productive play opportunities.

The clinical play theory leads us (ultimately) somewhat nearer the biology of play.

In work, where the normal consciousness is functioning predominately, the personality is limited by the real aims and conditions of the task. In play, however, which expresses the spherical effective life, the individual can, relatively unrestrained, reveal and develop his passions and wishes. It is the biology of play which permits the individual to use and to train all those creative forces which are ignored or limited by the necessary adjustments of the struggle for existence. Since childhood is a period of development, it is then in the periods of play that the child can most freely develop his creative potentialities. Through his parent's instincts, nature frees the child from the struggle for existence, from "work," in order to awaken and to develop through play, free creativity.*

* The well-known German scientist, Prof. Gross,[16] considers the play activity as a preparatory training for useful work in future life. This, however, cannot explain the impetus and passion with which children play, and the inexhaustible productivity of the free play fantasy. It is, biologically no less important to develop during childhood the creative activity and the emotional life-urge which build up the fundamental vigor of developing personality.

Only once in a lifetime, namely in childhood, the individual has both the ability and opportunity of that creative play fantasy which does not yet discriminate between the reality and the dream, and which senses both the essential reality of dreams and the dreamlike charm of reality.

Childhood is, therefore, the source and the reservoir of free creativity for the entire lifetime.*

Loneliness or playlessness produces a sickening deficit in the child's spherical intimate life. Individuals who did not play in childhood are apt to be in later years, under-developed in their spherical emotional life. They have on the whole less creative initiative and a weaker life urge.

Practically important are the following conclusions:

1. Rest periods, which follow work periods in adult life as recesses follow lecture periods in the school child's life, are not passive states of work cessation, but are, rather, active stages of spherical recuperation. Only then, in natural play diversion, the creative passions come to full expression. Not only the lecture period, but also the recess period stimulates, immediately, the child's intimate and emotional life. School pedagogy should, therefore, concentrate rather on the recess, than on the lecture period.

* Childhood is thus, not a period of lesser being, but rather of greater being of the personality, which later, in maturer years, becomes "captured" by reality (normal consciousness) and does not then dare to further bring out the untiring creative curiosity and urge of childhood. In psychoanalysis infantilism is synonymous with immaturity and primitivity. The studies of play and fantasy in children, however, show clearly that childhood is the prime creative essence of the personality, the basic capital assets on which we realize later in maturer years only partially and possibly. (See my book, "The Psychology of Intimate Child Life," Chapters 9-11, and my book, "The Catastrophical Epoch," Chapter 6.)

Note the similarity between our scientific conclusion and the poetic conception of Wordsworth.

> Heaven lies about us in our infancy!
> Shades of the prison-house begin to close
> upon the growing boy
> * * * *
> At length the Man perceives it die away,
> And fade into the light of common day.

(The Ode on the Intimations of Immortality From Recollections of Early Childhood.)

2. In work (normal consciousness) the individual wishes to do what he can in accordance with actual work conditions; in play (spherical life) the individual can do what he wishes in accordance with his inner urge. In the former, the personality is limited within the framework of adjustment possibilities; in the latter, the personality reveals itself in the unrestrained freedom of play. The study of play offers us, therefore, a methodic approach to the intimate emotional life. This can be proved experimentally, as we shall see in the following chapter by a systematic comparison of various games in different child groups. This will reveal in play a new method of investigation of the emotional life, at the same time that it will serve as an experimental proof of the clinical play theory.

VI.

Play as a Fantasy and Emotion Test of Child Groups.

By closer observation of plays, we finally arrive at the problem of play content, which proves to be an organic expression of the child's emotion and fantasy. To be sure, it is a very obvious fact. The passionate eagerness with which children play and the vivid fantasy with the help of which only children can imagine themselves and their playmates in different rôles and situations—all this clearly shows the organic relation between play, fantasy and emotion. By methodically comparing the content of different plays in different child groups, we can objectively ascertain and more exactly apply the play as an expression and investigation method (test) of emotion and fantasy. Let us first make a brief theoretical observation which would be productive for investigation and at the same time of practical significance.

As long as psychology ignored the organic connection between fantasy and emotion and had considered each of them separately—both were equally inaccessible for investigation, because each was torn away from its organic foundation. First the new investigations, among them also our experimental investigations of child fantasy,[17] showed that fantasy is an emotional creativity, *i. e.*, such mental activity that realizes our emotions in images. As a person feels, so he fancies. This is the principle of the twofold expression of emotions—the image and the motor expression. When we, for instance, love or hate a person, we, accordingly, have

an attractive or repulsive image or idea of that person (the image expression); at the same time we actively approach or turn from this person (motor expression).

Children's tales express their emotions in images; the play gives motor expression to these emotions in movements and acts of play. The stories and games are to children what the novel and the theatre (epos and drama) are to adults. In both cases the fantasy epically or dramatically expresses the emotions—in images or in motions. This can be formulated more exactly; stories and games are two forms of free fantasy, but differ in the following; the former is epic and individual—children mostly fancy for themselves and within themselves; the latter is dramatic and collective—children mostly play in groups. The story, therefore, expresses the individual emotions of the child; the play, however, expresses the emotions of the entire play-group.*

The fantasy is divided into two principal categories, according to the emotions which are thereby expressed. As is known, we distinguish between practical and free fantasy. The former realizes those emotions which are related to reality and demand the adjustment of or to reality. The fantasy must here find and fix such images as will practically lead to the end in the given situation. For instance, the scientist or the business man cannot fancy freely; they look for such combinations which permit them to actually dominate over their domain in science or business. We have elsewhere designated practical fantasy as bound, because it is bound by the reality which one wants to dominate by means of his fantasy.

The free fantasy expresses such emotions that are not bound to reality and freely express the passions of the individual. The scientist or the business man can in moments of leisure lend himself to intimate moods which are expressed in day-dreams and play. The practical fantasy dominates during work (normal consciousness), which requires a rational and purposeful attitude. The free fantasy dominates the play (spherical life), where chiefly the intimate passions find their expression. In the mental life of the child the

* There are some rare transitional forms of group stories and individual plays: A child can improvise stories for other children, thus transforming its images into a collective narrative play; on the other hand, a child can, though seldom, play alone, thus reducing its dramatic play activity into self imaginings.

free fantasy dominates over the practical, as the play dominates over the work in the child's spontaneous activity.

We can now make two important deductions: (1) Since the free fantasy expresses the intimate emotions in images, we, therefore, can investigate the emotions by means of images. As is known, emotions are inaccessible for introspection. As soon as we concentrate attention on our emotions, they become hazy and indefinite. Besides, we can scarcely "translate" or "describe" our emotions in words. It is still more difficult for children, who are still less able to observe and to "describe" their emotions. Emotions were, therefore, inaccesible for investigation. Fantasy images, however, are very plastic; they lend themselves to objective observation and description. The methods by which we observe the free fantasy as an expression of emotion may at the same time serve as objective methods of fantasy and of emotions. (2) Both forms of free fantasy—the epic narrative and the dramatic play—offer different approaches to fantasy and emotions. The former reveals the most individual emotions of the child: the latter expresses the emotional life of the entire play group and its participants.

We elsewhere [11] described the investigation methods of the epic narrative fantasy of children. We will now take up the play fantasy.

In our seminary for Social Child Psychology we used two investigation methods—the direct method, when we observe different plays of familiar child groups and we compare the games with the familiar character of these groups; in the great number of such investigations it proved without exception that the play reflects the emotional peculiarities of the group. The second method is the indirect method, which can serve as a controlling method. An example will illustrate the method: A member of the seminary, frequently a teacher, at the meeting reads a record methodically describing plays of some child groups, making no mention of age, sex or character of these groups, and on the basis of the play material alone, the seminary ascertained (according to the principles described below) the character of the group and thereafter this was checked up in reality and practically always it checked correctly. Every pedagogue can by means of these two methods observe any child-play whatsoever and establish our basic principle—that play expresses the emotional life of the child.

The play, as a dramatic expression of emotion, has its own construction, its specific means of expression. Through the comparative investigation of different plays and different conditions of various child groups, we can establish the following principles of play investigation:

I. *The Play Contents.*—This expresses the emotional character of the ***entire*** play group. In the above-given description (Chapter II) of a recess of normal children, we saw that the dog-play expresses the combative instinct of the adventurous boy group, as the rhythmic round-dances are characteristic of the tender girl group. The play is a group creation where children stimulate each other and join their expressions in the entity of the play content. Every play of normal children, as the drama of adults, has its finished content—the main motive, which unites the individual players and the different episodes in the unified entity of the play content. The play here expresses the nature of emotional life where different *by-motives* are united by an emotional main-motive in the multifarious entity of the personality. The by-motives lend individual color to the dominating main motive, as the overtones give the specific timbre (shading) of the tone. In the content we must, therefore, distinguish between the main motive and the by-motives in their various significances.* The chaos or dissociation of play content is a symptom of a corresponding emotional abnormality of the play group. In the case of morons, as we saw above (Chapter II), the play has practically no content, being for the most part chaotic. Where children are excited, over-fatigued, or "nervous," their play accordingly become unco-ordinated and dissociated.

The principle of play content is properly the principle of harmonic group creativeness of normal play.

* The combative motive can be a main motive in different plays—In competitive sports, in the above-mentioned dog-game, in the burglar-and-police game; in each of these games, however, the main motive of combat reveals its individual color; in the game of sport it is the urge to motion, body discipline, organic sense of rhythm, etc.; in the dog-game—the primitive prey emotion of biting each other, the joyful domination of fear through self-transformation in the horrifying appearance of wild dogs; in the burglar-and-police game—the adventure-urge of thieves and of thief-hunters, the whole emotion complex of crime and punishment in the feeling of the hiding thieves and the severe official enforcers of order; etc.

II. *The Rôle in the Play.*—Each player can choose his rôle and thus express his individual emotions in the general group play. In the above-mentioned combative play of the boy's group, it is very characteristic that one takes the dominating aggressive rôle of the attacker, and the other—the submissive rôle of the attacked. When, for instance, boys play horses, it is very characteristic, almost striking, that one for the most part plays the rôle of the rider, and the other—the rôle of the horse. These are different characters which express their emotional peculiarities in the various rôles.

It is interesting to note that some neurotics have sharp memories of the steady parts they played in the games of their childhood.*

The play of children, as the drama of adults, has a differentiated structure which allows each participant to express himself individually in the harmonic entity of the play.

Lack of an assignment of parts in the play is a sign of subnormality of the group. The morons (Chapter II) have not yet developed to rôle assignment.

The principle of play parts is properly the principle of differentiation of normal play.

III. *Nuances in Play.*—Each child can put various nuances into a rôle and thus express his individuality. When the game assigns similar rôles to different children—still, each child shades the same rôle in its own way. In the above-mentioned record of the recess in school, we see that in the game of " wind-mills " all the children move seemingly alike in the rhythmic round dance, but Malkale introduces a new characteristic nuance (with the tips of her fingers she takes hold of the hem of her skirt, bending her head to one side), which expresses her graceful girlish coquetry.†

* Here is a typical case: L., 42 years old suffers from a compulsive anxiety for other people, blushes and trembles in the presence of others. Among other things, he remembers that in his childhood he would always play submissive parts (the horse, the captured burglar, etc.).

† Here is a striking instance of play nuances: Two children, a boy of nine and a girl of eight, play ball. The boy is aggressive, adventurous, witty, a good student; the girl is tender, submissive and harmoniously talented. We will now describe the play, without mentioning who the part-takers are, but the reader will be able to recognize them by the play nuances. The players are A. and B.—A. throws the ball smilingly and with elastic movement to B., who catches it trickily on the back of his head and bounces

The normal play is always elastic and rich in nuances and never has the mechanical monotony of those organized games which children play by order, without will or initiative.

The principle of play nuances is properly the principle of modification.

IV. *Play Procedure and Play Finish.*—These decisively show the true nature of the entire game and of each rôle. The game is a dynamic process which develops characteristically in the course of its procedure. Whether the play has a slow or stormy tempo, whether the procedure of the play has an ascending or descending tendency, whether the play is monotonously repeated or constantly renewed—all this indicates the true nature of the play.

We frequently observed how a fighting game played by girls lost its fighting spirit through weak tempo and monotony, while an ordinary round dance in a group of boys was transformed into a stormy competitive dance.

Following is one of our many typical observations: Two boys are playing horses. The older boy, aged eight, strong and ambitious, nevertheless takes the part of the horse; the younger, aged 7, physically weaker and submissive, takes the part of the rider, At the start, the rider rode very cheerfully astride on the galloping horse, triumphantly blowing a trumpet. "Suddenly," the horse turned wild, and loudly neighing began to run around stormily, cruelly bouncing and shaking the rider up. The rider cried for fright. The elders rushed in, and the horse explained naively that they were simply playing "the rider on a bronco." The submissive part of the horse in the course of this play is transformed into the aggressive, adventurous one.

A normal game is always dynamic and has its development. Stereotypy—*i. e.*, the lack of dynamics—is a symptom of subnormality. Imbeciles and morons show different grades of stereotypy. The principle of play procedure is properly the principle of dynamics of play.

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it back to A. The latter smiles still more at the tricks of B. and throws the ball again with loud laughter. B. now bends over a little and catches the ball in his woolly hair, which calls forth further loud laughter from A. In this way, each new bounce of the ball called forth a new trick by B. and the greater laughter from A.

Of great importance is the principle of play finish. In many cases not only the general play content or the main motive, but the play finish, or the " last " play motive is decisive. This is explained by the nature of the emotional life which finds its dramatic expression in play. In the emotional life of the individual or the group, there can be different, even contradictory, emotions, which however, are dominated by the main motive of emotional life, and thus is created the multiferous unity of the personality. These contradictions, though combined and dominated, dynamize and shade the personality. Not infrequently, however, there happen in the emotional life conflicts of contradictory emotions, where the personality sways between two or even more main motives—problem personality! Here the finish, the last, the most weighty motive, is decisive. For instance, a person can have so strong a feeling of duty that the desire to betray never enters his mind. The desire of betrayal may at times be sufficiently strong to enter the mind, but it is checked at once by the stronger feeling of duty; but should the desire of betrayal come into conflict with the feeling of duty, then the " last " motive, the more weighty of the two, is decisive—the person " becomes " a betrayer or " remains " a man of duty.

In the game of horses mentioned above, the last motive of the wild horse points out the true nature of the game. Our investigations of the epic narrative fantasy of children have shown [18] that in children's tales, as in their play, the last motive is the decisive characteristic of the child's emotional life.

To be sure, the principle of the last motive is universal. The moving picture theaters, for instance, usually show tragic or melodramatic pictures, in order to satisfy the desire for emotional tension; however, they usually have a happy ending which satisfies the most weighty striving for peace and harmony in the average man.

On the higher creative level we find the principle of " last " motive in Beethoven's Ninth Symphony, where the tragic motives of the beginning finally are dominated by the ecstatic finish. Here is shown the deeply entwined web of Beethoven's intimate life, where the ecstasy is the most weighty, but not the only, dominating motive.

SCHEME FOR INVESTIGATION OF PLAY AND GROUP LIFE OF CHILDREN.

1. Name of investigator.....2. Date.....
3. Time of day.....4. City.....
5. School (family or local collective).....
6. Number of children: Boys.....Girls.....7. Ages: From...To...
8. Number of groups formed.....
9. Number of lonely children.....
10. Characteristics of each group:
    a. Number of children in the group.....
    b. The leader in the group.....
    c. The fight for leadership.....
    d. Actual relations between leader and individual members of the group .....
11. Characteristics of the group play:
    a. Duration of play.....
    b. Content of play { Main motive ..... / By-motive ..... }
    c. Parts .....
    d. Nuances .....
    e. Procedure of Play.....
    f. Last motive of play.....
    g. General conclusion of the play characteristic.....
    .....
12. Characteristics of each lonely child:
    a. Behavior .....
    (Passive observer, wandering among groups, disturber of order, secluded, etc.)
    b. Causes of loneliness.....
    (Physical weakness, backwardness, depressiveness, bashfulness, restlessness, social inferiority, educational disturbances, etc.)

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A NOTE ON ADMISSIONS TO STATE INSTITUTIONS.*

By HERMAN ADLER, M. D.

In a study of the social status and geographical distribution of admissions to the state institutions of California two observations were made which appear to the writer sufficiently important to bring to the attention of the Association. The accompanying charts will illustrate the points.

1. Overcrowding of the institutions made necessary an estimate of the number of patients who would enter the institutions within a given period. An examination of admission rates and trends disclosed the surprising results that if the directions shown by the admission rate in the past 20 years continued, there would be a considerable diminution in the rate, although there would be an actual increase both in the population of the state and in the number of admissions. Further investigation may disclose that the apparent decrease in the admission rate forecast will not be borne out by events. Nevertheless it seems pertinent to raise the question whether the admission rate of mental disorder is not dependent upon the capacity of institutions rather than upon the incidence of these disorders in the community. When new institutions, or additions to existing institutions have been made, the admission rate has risen, then presently fallen off again, when the pressure is relieved.

It is not possible, apparently, to maintain any institution at the population for which it is designed, but due to pressure from without a state of overcrowding must result before the internal resistances rise to the point where equilibrium can be established. This condition prevails in both the institutions for the insane and in the institutions for the mentally deficient.

The assumption, based upon these figures and observations, is that the admission rate is not indicative of the true state of affairs in the community relative to mental disorders, nor is the state of

* Read at the eighty-eighth annual meeting of The American Psychiatric Association, Philadelphia, Pa., May 31–June 3, 1932.

overcrowding an index of the needed bed capacity. It seems to be a common experience that when overcrowding has been allowed to progress to such a state that the safety of the institution is menaced and new beds are provided in the old institutions, there is an influx of patients, rather rapid at first, which continues until overcrowding and relative stagnation again occur.

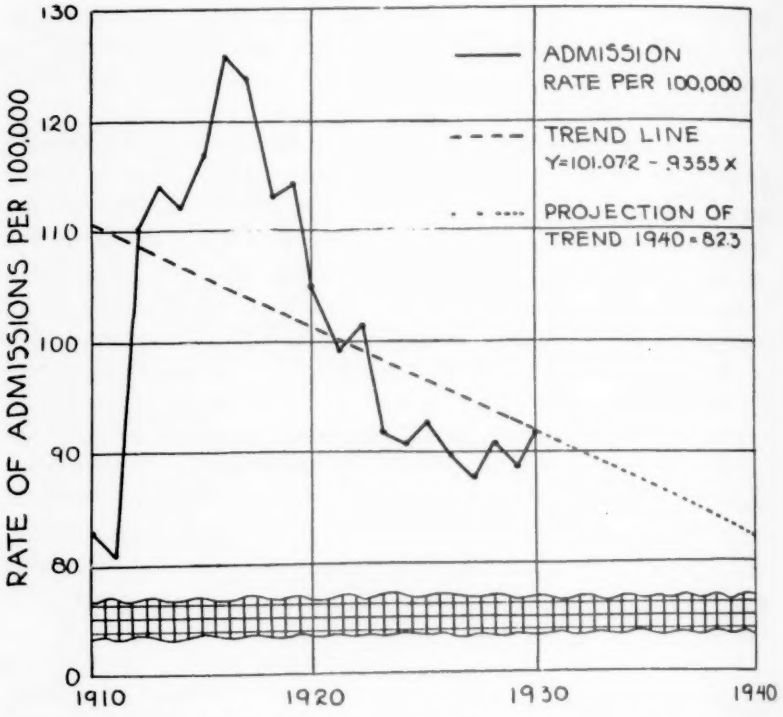

CHART I.—Trend of Admission Rate to California State Hospitals 1910-1930.

2. The second point has to do with the character of the admissions to the institutions. Examination of the average admission ages to the different kinds of institutions indicates that there is a definite separation by decades of delinquents, criminals, and the insane. It is interesting to note that delinquency predominates in the adolescent period, that criminality is primarily a phenomenon of young adult life, and that mental disease is an affliction of middle life. Accord-

ing to these figures, there is a close correspondence between the figures obtained for California and those for the country as a whole.

This chart would seem quite regular if mental deficiency appeared on it with an average admission age within the first decade of life. Theoretically, one would expect that, since mental deficiency is the one disorder above all others which should be diagnosable at any age on the basis of very definite criteria. These criteria, as is well known, consist of mental tests arranged according to various rating

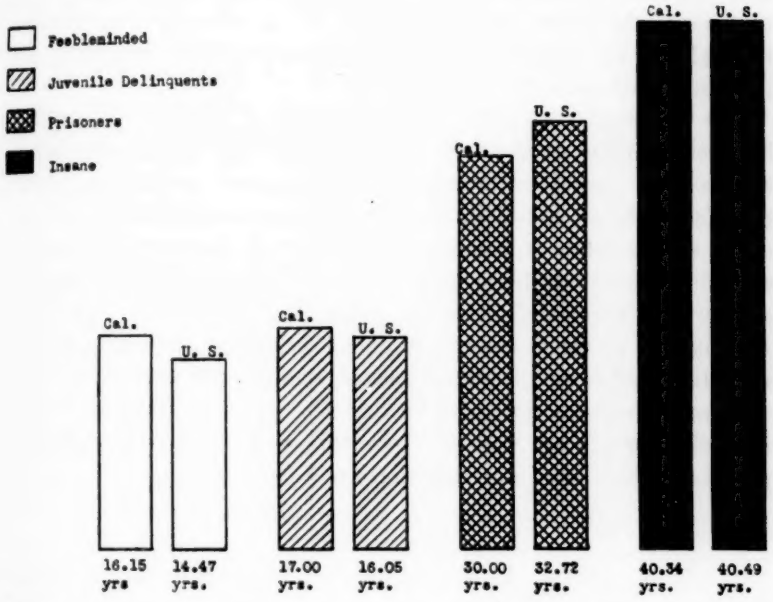

CHART II.—Average Ages of Persons on Admission to State and Federal Institutions, California and United States.

scales, which are particularly successful at the ages of 6 to 14 years. One would have a right, therefore, to expect that the frequency of the diagnosis of mental deficiency and the average age of admission to the institutions for the feebleminded would fall in the decade preceding adolescence. Such apparently is not the case. The average age of admission of the mentally deficient is only slightly below that of admission to the correctional institutions.

In spite of all that has been said and written on the subject it would seem that the decision as to whether a person is sent to a

school " for special training " does not rest upon the diagnosis of feeblemindedness, but upon the social behavior of the individual. Whatever may be true of the older feebleminded person, the feebleminded child apparently is able to get along at home and in the community until the early adolescent years. At this time something happens, and only slightly earlier than in the case of the non-defective delinquent, his case attracts attention. After more or less serious attempts to deal with him have failed, he is brought into court. If his mental rating turns out to be low enough, he is sent not to the correctional school, but to the school for the feebleminded. If his mental rating is not low enough he is sent to an institution for delinquents.

The question naturally arises, is there a similar relationship between the other groups here dealt with. Is there a common factor that is to be found in these three or four main categories, and is that factor to be found in the aspect of anti-social behavior? In other words, when it comes to treatment, is not the problem of discipline the underlying denominator? The manifestations of unruly and anti-social behavior differ with the age of the individuals, and so we classify them under different categories. If this is so, the further question is suggested, namely, are we dealing with the same processes operating in different ways in each individual according to his age, or are we dealing with entirely different processes which have a predilection for different ages?

DISCUSSION.

DR. HORATIO M. POLLOCK (Albany, N. Y.).—Mr. Chairman, I am much interested in Dr. Adler's comprehensive study. Our findings in New York State in regard to the trend of admissions to mental hospitals do not correspond closely with those shown for California by Dr. Adler. We have an accurate yearly record of the first admissions to all hospitals for mental disease in the state since 1909. In that year the general rate of first admissions per 100,000 was about 65; in 1931, it was approximately 77. The increase has been fairly constant, although there was a slight decline in the early part of the past decade and a marked rise in 1928. We have tried to analyze the rate to find where increases or declines were occurring. In the syphilitic psychoses there has been a slight decline since 1918. In the alcoholic psychoses, there was a decline between 1910 and 1920. After 1920, the trend was upward to 1927, and since that time it has remained practically on a level, still considerably lower than that of 1910. Our greatest increase has been in the arteriosclerotic group. In 1912 we had a rate of first admis-

sions in this group of a little less than 2 per 100,000 of population, but in 1931 the rate went up to over 10. The number of first admissions increased from 166 in 1912 to 1376 in 1931. We have also had an increase, although not large, in traumatic cases. That is due to the larger number of automobile accidents. There has been an increase in encephalitis cases and a slight upward trend in dementia præcox. Changes in rates of first admissions in other groups have not been marked.

Our admission rate in recent years has undoubtedly been affected by the depression and by our building program. As conditions vary greatly in the several states, the trend line shown by Dr. Adler is not applicable to the country as a whole.

CHAIRMAN HAMILTON.—Are there any other discussers? Having had the privilege of reading Dr. Frederick Allen's admirable survey of California, I wonder whether when the building program goes on and certain obvious advances in the standards of some of the hospitals in California are made, the admission rate won't go up. But I offer that as a question.

Will you close the discussion, Dr. Adler?

DR. HERMAN ADLER (Berkeley, Calif.).—I will say that we share that belief in California, Dr. Hamilton, and I feel I am particularly fortunate to have Dr. Pollock here this afternoon. I hoped he would comment on this. I am not sure that Dr. Pollock's remarks, however, do not support our suspicions here, because New York State, we know, has had a very splendid building program; it is one of the few states that has kept up with the admission pressure and, therefore, I expect the rate to remain either constant or to show these increases. The reason for bringing this in was not to make any generalizations but to point out that there were other factors besides the diagnostic ones which enter into the matter of admissions. I think that still remains to be studied further.

Whereas the Founder believes that Service to Others tends to fill Life with Joy and renders whole Communities prosperous and that the Ideal of Service as a test of good Citizenship should be kept constantly before the Minds of the People of Philadelphia in general and of the Young in particular and further believes that this may in some measure be accomplished through the making - under proper conditions - of an Annual Award in recognition of some Service rendered by a Philadelphian which shall have redounded to the Good of the City

• The •

Philadelphia Award

founded in 1921, is herewith made, for the Year of 1932, to

Doctor Earl D. Bond

Board of Trustees

Notes and Comment.

THE TWELFTH PRESENTATION OF THE PHILADELPHIA BOK AWARD.

On February 8, 1933, in the Academy of Music in Philadelphia there occurred an event of outstanding significance in American psychiatry. It was the Twelfth Presentation of the Philadelphia Bok Award. This particular presentation will live in the minds of American psychiatrists in its twofold significance. In the first place it did a justly deserved honor to an eminent psychiatrist who has for many years stood for the very best thought and practice in our specialty—Dr. Earl Danford Bond. In the second place it will always stand as the most signal recognition of the important rôle that psychiatry and mental hygiene play in the life and progress of our civilization.

The Philadelphia Bok Award is an award of a medal, scroll and ten thousand dollars in recognition of the service of that citizen of Philadelphia who has rendered the most noteworthy service to his fellow citizens during the past year.

Dr. Bond's name and accomplishments, therefore, are recorded along with eleven other notable first citizens of Philadelphia.

The exercises were opened by Mr. Franklin Spencer Edmonds of Philadelphia acting for the Trustees of the Award. He gave a splendid address but purposely refrained from any mention of the recipient-to-be. This was followed by the address of Mr. Roland S. Morris, Esq., a noted lawyer of Philadelphia and the former Ambassador to Japan. Mr. Morris said in part:

> And as the Trustees, during the past year, considered the conditions of our community life they felt that it was not only in the field of the physical that men have carried on. They found that among the institutions there had grown up a work which was simply revolutionary in its effect in one great field of service to mankind. They learned that there had been established in Philadelphia, as a position of one of our own hospitals, an institution which was the outgrowth and the produce of almost completely radical changes of method and approach in the study of one of the most baffling problems of humanity—the problem of the mentally ill.

And when I try to describe that method, as we discovered it and as the layman would express it, it was based on this conception, that we are beginning to realize today that we have as it were a personality that is common to all mankind, a mind that lives only as it lives in the minds of others, and a consciousness that is shared by all human kind as its great heritage—not by one alone but by all of us—penetrating one into the consciousness of another. With the development of modern psychiatry we have begun to learn as never before the depths and meanings of the personality, conscious or unconscious, and we have begun to learn something of the meaning of mental illness. We can now see that inherent in all of us are qualities which in exaggeration become abnormal so that you and I can better study our own normal life if we can study it in terms of those who abnormally develop one aspect of their life at the expense of others. This has meant the blending of all that has been learned in modern psychology by psychoanalysis, by the study of abnormal cases, by the study of children. We have been able to get standards of normality that have brought health and happiness and hope into thousands of lives through the technique which has been a product not of one man but of a number of research scholars.

As he concluded, with the mention of Dr. Bond's name, there was a tremendous ovation from the large representative audience and Dr. Bond received the Award.

In his reply Dr. Bond made the following remarks:

It is indeed true that one can never tell what one's friends will do to him and will say about him; and for some days, I have been praying for a sense of proportion in this place in which my friends have put me. I suppose that every representative of a group must feel, in such a situation, that the Award ought to be more widely distributed.

The building of the Institute in which I work was done by Dr. Owen Copp, my former leader and the ablest constructor I have ever known. The clinical foundations of our work were laid by Dr. Edward Strecker, my friend and associate for twenty years, in his work at Eighth Street and Forty-fourth Street; but Dr. Copp has removed himself to the south of France, and Dr. Strecker has taken himself to that psychiatric teaching which is his heart's desire and has removed himself from organization responsibilities. This has let this honor fall upon me, who have often thought of myself as the tie which bound them to each other and to the younger generation.

May I take you, for just a moment, on a kind of psychiatrical Pilgrim's Progress of my own experience for twenty-five years. I turned first to the study of mental diseases and then I found a much more interesting thing—the mental patient. Personalities unrolled themselves before me and challenged every cut and dried idea I had ever had. I found some of these patients were too logical for this practical world of ours; others regarded outward success as inner failure; some were so simple that they reminded me of children, and the more I turned to the study of children the more I saw the likenesses between mental patients and children. The next step was more of a shock; the

more I saw of mental patients, the more I saw that they were like myself; their virtues and their vices were mine but their's were written more large, and then as at one time I had the chance to see genius in action I found that those men and women were really closer to mental patients than they were to me.

These steps led all to one conclusion, we were all in the same boat—mental patients, children, myself and other ordinary people, gifted people, geniuses. What are we to do about it? Certain fundamental ways of thinking and doing run through us all. Now, my own solution is to turn to mental patients as teachers. They say this: "There is something in our minds that has not kept pace with our physical and intellectual growth; that lagging part of the mind holds our emotions, our fixed attitudes, our tangled loves and hates."

As a result, we see the finest men and women going about with unnecessary loads of unhappiness, without knowing why they walk always in the shadow of fear and guilt, and as a final result, they cannot lead in social relationships as they lead in scientific advances; they can manage the most complicated series of machines better than they can manage their own relations with their fellowmen.

Now, the Institute was founded to help balance normal minds. It is possible that in the reconstruction which is coming, of which Mr. Edmonds spoke, the ideas from balanced minds will be of more importance than material resources.

Tomorrow we shall face the world with new powers. Mr. Bok's generosity will make it possible for us to reach many more people who need help and who cannot pay. The sympathy and the understanding of twelve far-seeing men and women will give to us that inspiration which is so needed by our staff and by our devoted Board of Managers.

I accept this Award as a contribution to the study of the open mind in Philadelphia and I accept it, not as a reward for things done but as a very tactful suggestion of how much more ought to be done.

As a friend and fellow-worker of Dr. Bond's I know that I am voicing the consensus of opinion of American psychiatrists that we all rejoice in having this proper and public recognition of psychiatry and are happy that it was bestowed upon a man so eminently fitted to receive and wear the honor.

EDWARD A. STRECKER.

NARCOTIC DRUG CONTROL. THE GENEVA CONVENTION OF 1931.—The international treaty for limiting the manufacture of narcotic drugs will become effective July 13, 1933. It is the result of an international conference held at Geneva from May 27 to July 13, 1931. Of the 55 nations sending representatives to that conference, 35 had ratified or acceded to the treaty on April 10 of this year.

The United States Senate on March 31, 1932, was the first to ratify the convention. The American delegation deserves signal

credit for negotiating an international agreement that should be far reaching in its effect in preventing the smuggling of contraband drugs into this and other countries. One problem of narcotic drug law enforcement in the United States has been the prevention of the unlawful introduction of drugs from abroad. Domestic manufacture in our country is limited so closely to the medical needs that diversion of home manufactured products is comparatively negligible. Foreign overproduction, however, in excess of the amounts needed for medical and scientific purposes, supplies the international illegitimate trade. It has been impossible, therefore, for the United States to set its house in order in this regard without the cooperation and assistance of other governments for limiting the manufacture of narcotic drugs to medical and scientific purposes.

The treaty does not weaken or relax existing American measures for the control of the legal use of these drugs or those for the suppression of their abuses. It deals with two phases of the narcotic drug situation, first, the limitation of their manufacture; and second, the strengthening of measures for their international control and disposition. In the terms of the treaty each country submits annually to a supervisory body an estimate of its domestic needs for narcotic drugs based on medical and scientific requirements. World manufacture, therefore, must not exceed the total quantity shown by these estimates. Importation and exportation are restricted with a view to preventing an accumulation in any country of any quantity of manufactured drugs in excess of actual medical and scientific requirements. In the field of international control the system provided in the treaty approaches in general principles our own national system of drug control.

The Geneva Convention of 1931, if honestly and faithfully enforced by all countries, together with existing measures for domestic control, should play an important part in helping to solve the problem of drug addiction in this country. It will serve to protect the Western Hemisphere against the avalanche of contraband that, in the past, has emanated from countries manufacturing narcotic drugs in excess of the medical and scientific needs of the world.

However, experiences of the past have taught that depraved men and women are eager and ready to raid those supplies destined for medical and scientific uses when contraband narcotic drugs are

not available or are unusually difficult to procure. They will seek every means to divert such supplies from legitimate channels. The adoption of a public policy of segregating, isolating, and treating drug addicts as a means of offsetting this potential menace to the legal supply of dangerous habit-forming narcotic drugs should serve to safeguard the interests of those professional and business groups who are custodians of these drugs. Restrictive laws governing the commerce in narcotics are not, however, the only measures to be applied in the solution of the medico-social problem of drug addiction. The need for destroying drug peddling and its source of supply is obvious, but the potential market for contraband and the menace to the legal supply of drugs must not be neglected.

W. L. TREADWAY.

Association and Hospital Notes and News.

1933 Social Work Year Book.—We have been favored with advance sheets of that section of the 1933 Social Work Year Book, published by the Russell Sage Foundation, which treats of mental hygiene. It is of interest that so extensive a subject has been covered so well and that so much information has been compressed in these four pages. Numerous references to other subjects treated in the book give further opportunity to amplify the information contained in this section upon mental hygiene. After reading it, those interested in social work may feel that they have considerable knowledge of this branch. The definitions by Howard Becker and Dr. C. Macfie Campbell are admirable in their succinctness and clearness. The writer, Clara Bassett, is to be congratulated upon performing her task so well.

International Hospital Association.—The Third International Hospital Congress will convene at Knocke sur Mer (Belgium), June 28 till July 3, 1933. All officials, associations, institutions, and persons who are interested in the construction, administration, and development of hospitals are invited to attend. The world crisis has placed the tasks which the hospitals of our day have to fulfil in such a strong and clear light that the necessity has made itself felt more than ever of exchanging the views of the different countries, and coming to an international understanding on the important issues involved. The reports of the *10 International Study Committees* will be laid before this Congress. These have been arrived at by an exchange of views by outstanding specialists of different countries. These reports will contain suggestions for common international principles, their purpose being not to form one uniform system, but to bring about a general perfecting and improvement in the hospital system.

The references for these committees are: 1. Construction. 2. Equipment and technique. 3. Administration and house-keeping.

4. Finance and book-keeping. 5. Legislation. 6. Care of the patient in the hospital. 7. Patients' food. 8. Personnel. 9. Statistics. 10. Outside connections with the hospital.

All the reports which will be read before the Congress will appear in the April number of *Nosokomeion,* the official organ of the Association. The final reports as adopted by the Congress will be forwarded to the League of Nations and to the governments concerned.

Following the scientific sessions there will be a five days study journey through Holland (July 4-9), which will enable the Congress to see not only the new hospitals but the classic places, the centers of industry and scenery of Holland.

Notifications should be addressed to the Belgium Hospital Association (Association Belge des Hôpitaux), 80, rue de Livourne, Brussels, together with a contribution of two dollars. The members of the International Hospital Association who pay a yearly contribution of five dollars may attend the Congress without further payment. In addition they receive the organ of the society, *Nosokomeion.*

All communications, questions, reports, and suggestions should be addressed to Dr. W. Alter, Ernst Ludwig Allee 2, Buchschlag, Hessen, Germany.

Detailed programs can be obtained free of charge from Messrs. W. Kohlhammer, Verlag, Urbanstrasse 12/16, Stuttgart.

THE COMMONWEALTH FUND, FOURTEETH ANNUAL REPORT, 1932.—In spite of the depression the Commonwealth Fund has maintained most of its activities and reports the expenditure of $1,594,888.09 in the past year. Edward S. Harkness is president of the Fund, established in 1918 by Mrs. Stephen V. Harkness " To do something for the welfare of mankind." Some of the year's work is enumerated below.

The Division of Education—The Commonwealth Fund Fellowships.—Each year since 1925 a group of British students comes to America for a period of post-graduate work. This fellowship leads to a greater friendship between the two nations and develops a better understanding of American problems on the part of the British students.

Rural Health and Rural Medicine.—The Fund shares in the development of rural health service. It has aided state and county health departments in specific undertakings in various sections of the country where need has been greatest. It has also fostered the development of rural hospitals, provided post-graduate training for rural physicians and scholarships for selected undergraduate medical students to enable them to prepare for rural practice.

Mental Hygiene.—For ten years the Fund has devoted a large share of its appropriations to the furtherance of mental hygiene, chiefly through the medium of the community child guidance clinic. Training for the professional fields which come together in the clinic—psychiatry, clinical psychology and psychiatric social work—has been promoted along with this work. As the clinics have spread the interest of the Fund has gradually shifted to the broader problem of psychiatric education in general.

The child guidance clinics hold their own even through these years of depression. Only one clinic—that of New Orleans—has closed its doors. The depression affects the clinics through the changes it has wrought in social work in general. But this makes the clinic a more flexible instrument applying its resources in a variety of ways. Much consideration has been given to questions of community affiliation, looking especially to the establishment of closer relations between clinics and larger institutions including hospitals, medical schools, social agencies and public schools.

Training: The Institute for Child Guidance.—Training has taken two directions:—the provision of field training for students of psychiatric social work, and the preparations of psychiatrists and psychologists for staff positions in the clinics. During the past year the institute also opened its doors to psychiatrists who came for short periods of observation.

Training: Psychiatric Education.—When the child guidance clinics were new, and most of the psychiatrists in the field had been trained primarily for state hospital service, it was the exceptional man who was ready to step into a position where he must deal with the nascent disorders of childhood. Later fellowships for post-graduate work were offered at teaching centers to ensure the necessary equipment for such work. There is opportunity now for a fresh approach to the foundations of psychiatric training—instruction in pre-medical and undergraduate medical curriculum.

This the Fund has fostered through a share in the support of the Division of Psychiatric Education, organized in 1931 by the National Committee for Mental Hygiene. Thirty-five fellowships for advanced study in psychiatry at various leading institutions have been provided.

Child Guidance in Great Britain.—Although more recent than in the United States, the development of child guidance is very successful in Great Britain. Before child guidance clinics appeared the nearest equivalent was to be found in hospital out-patient services where the psychiatrist worked almost single-handed. The gap between psychiatrist and social worker was a wide one. Now experimentation in team work goes on continuously. Even so, the process of integrating the three professions—psychiatry, psychology, and psychiatric social work—is slower than in the United States. The rôle of the psychologist in the British clinic team, on the other hand is larger than that of his American colleague. The psychologist takes an active part in adjusting school difficulties by individual coaching of children. Another extension of psychological service at this clinic—uncommon in America—is found in the play-grounds which meet twice weekly for young children and once for older ones. Here the reaction of children to play-material and to each other can be watched and children who fail to get on with their kind can be weaned from isolation.

The Division of Publications.—This is a non-commercial publishing unit which serves all the operating divisions of the Fund. Among the new books published during the past year were: *Child Guidance Cases,* edited by Mary B. Sayles, a compilation of case records selected from the files of several child guidance clinics for teaching purposes; *Directory of Psychiatric Clinics in the United States,* listing and describing all the clinics for both children and adults known to the Division on Community Clinics of the National Committee for Mental Hygiene; *School and Mental Health,* bringing together seven articles of Clara Bassett; *Child Health and the Community,* by Courtenay Dinwiddie and *Community Health Organization,* edited by Ira V. Hiscock—both on the subject of public health; *A Standard Classified Nomenclature of Disease,* edited by H. B. Logie, M. D. for the National Conference on Nomenclature of Disease, which presents the special piece of work financed by the Fund. This volume is intended to make available to hospitals and

private practitioners a uniform scheme for the identification and recording of diseases, and should facilitate not only accurate record-keeping but sound procedure in diagnosis. It is very inclusive and takes into systematic account both the etiology of the disease and the part of the body affected.

Although appropriations by the Fund were less than in other recent years, the special grants for 1931-32 reached a grand total of $362,791.26.

DR. KIRBY AND DR. CHENEY.—Dr. George H. Kirby resigned on February 1 as Professor of Psychiatry and Executive Officer in the Department of Psychiatry in the College of Physicians and Surgeons of Columbia University. Dr. Kirby has been appointed Consulting Psychiatrist to the New York Hospital.

Dr. Clarence O. Cheney who succeeded Dr. Kirby as Director of the New York State Psychiatric Institute and Hospital when Dr. Kirby retired in August, 1931, has been appointed by the trustees of Columbia University to succeed Dr. Kirby as Professor of Psychiatry and Executive Officer in the Department of Psychiatry of the College of Physicians and Surgeons.

Book Reviews.

The Sex Technique in Marriage. By ISABEL EMSLIE HUTTON, M. D. (*New York: Emerson Books, Inc.*)

It is highly probable that were every young couple given instruction in sex relationships, such as is given in this book, there would be much less marital dysharmony, fewer chronic invalids and mental breakdowns. The author is to be congratulated upon conveying information fully and sensibly. It is a straightforward discussion which even the most tender-minded may follow with complacency. The book is highly recommended.

W. R. D.

Recording and Reporting for Child Guidance Clinics. By MARY AUGUSTA CLARK. (*New York: The Commonwealth Fund, Division of Publications, 1930.*)

The author has compiled a very useful handbook for the keeping of records and preparing of reports. Although evolved from the work of the child guidance clinics, it offers a complete system of record taking which is applicable to any sort of medical clinic. The illustration of forms makes a clear presentation of the subject matter. I recommend them to any one concerned with the recording of examination and facts in a medical clinic. A great deal of detail is presented: No one clinic should need it all, but any clinic should be able to supply the needs from the material presented.

ESTHER L. RICHARDS.

Neuropathology: The Anatomical Foundation of Nervous Diseases. By WALTER FREEMAN, M. D. (*Philadelphia and London: W. B. Saunders Company, 1933.*)

For a long time there has been a definite need for a thorough, comprehensive book on neuropathology in the English language and we must be grateful to Freeman for having undertaken this task. The monograph in no way compares with the magnificent works on neuropathology published in Germany by Spielmeyer or Jacob, but it serves its purpose as an introductory volume. The first chapter deals with cytology and cytopathology where the author brings up to date the recent work on neuroglia which is still comparatively little known in the English speaking countries. Chapter two is devoted to the meninges, and there is a very authentic discussion of vascular diseases and arteriosclerosis in chapter three. The inflammatory diseases are discussed in the subsequent four chapters with separate chapters devoted to tuberculosis and syphilis. The recent work on Lissauer's paralysis is brought up to date. The

chapter on injuries is especially good where the author expresses fairly radical views on the pathology of cerebral concussion. In the discussion of the functional psychoses the author advances his own theory of dementia præcox as a deficiency disease and emphasizes the lack of iron in the cortical ganglion cells as a cause of the disease, as the iron is a catalyst necessary for the utilization of oxygen in the metabolism of the brain. In the manic-depressive psychoses the problem is again chemical rather than histopathological. The author very conservatively remarks that the neuropathology of the "functional psychoses" is yet to be written, but the use of quotes when he speaks of the schizophrenic and affective psychoses as "functional," suggests his fundamental point of view. The discussion of senile and presenile dementia is supplemented by excellent illustrations of senile placques and gliosis which tell the story very vividly. It is to be regretted that the note on lobar sclerosis does not contain illustrations of the cellular pathology of the disease. The last part of the book is devoted to the discussion of the degenerative diseases of the brain, and brain tumors. There is an appendix containing a glossary of the various terms, which is very helpful. The point of view of the author is purely histological rather than physiological, a point of view which I feel at the present time is rather unduly conservative. It is to be regretted that nothing is said about the pathology of the spinal fluid but perhaps the limitations of a small volume did not permit the author to enter into the discussion of the pathological physiology of the nervous system.

On the whole one can commend the author for writing an excellent monograph which is authentic, thorough and modern. The photographs are excellent, the book has been edited very carefully and one can see a great deal of care in preparation of the volume.

JACOB KASANIN.

Les Malades Dits Imaginaires. By M. NATHAN. (*Paris: G. Doin & Co., 1931.*)

This monograph from "Collection des Actualites de Médecine Pratique" gives a very good presentation of the modern French approach toward the psychoneuroses, as the chapter headings will show—the Cenestopathies, the Obsessions and Phobias, Hysterics and Hypochondriacs.

The author sets out to show that Moliére very likely did an injustice to his "Malade Imaginaire" in the light of present-day knowledge of body chemistry, especially as it is influenced through the autonomic nervous system and the endocrine glands.

With all due respect to these findings, one can doubt their importance in the total picture; certainly from the standpoint of therapy. The multitude of specific medications which are indicated for these conditions is not in accord with the best psychiatric practice in this country. This is especially well seen in the next to the last paragraph of the book in which the author advocates the use of peyote because "it can give good results in procuring for the patient a sentiment of euphoria, quite advantageous for psychotherapy." This

will serve to show the divergence from current American practice, where every effort is made to stay away from pharmacological crutches in the treatment of the psychoneurotic.

The book is very readable and gives a good bird's-eye view of the literature.

ESTHER L. RICHARDS.

Psychopathology of Forced Movements and the Oculogyric Crises of Lethargic Encephalitis. By SMITH ELY JELLIFFE, M. D., PH. D. (Monograph Series No. 55.) (*New York and Washington: Nervous and Mental Disease Pub. Co., 1932.*)

In this monograph Dr. Jelliffe deals with an interesting phenomenon whose history he touches upon in such way as to make one wish to follow it more fully. The brief historical introductory chapter has a facsimile of the first page of Albrecht of Hildesheim's observation in 1695 together with that writer's description of the case. The author does not attempt a detailed historical account of the problem of tics, but refers to the new interpretation placed upon psychical events since the days of Charcot.

Then follows the "résumé of the literature in detail" which occupies one hundred and ten pages of this little book. A thorough digest of cases with illustrations reported by one hundred and eighty-five authors from all parts of the world makes up the principal factual part of the book. Four of the author's own cases are reported. Then follow chapters dealing with symptomatology and psychopathology. Among the symptoms attention is paid to eye movements, thought developments, anxiety and hypervigilance. He says: "These oculogyric crises, *per se,* are not phenomena present only in post encephalitic patients but have been known to occur in epileptic attacks, psychotic states, in neuro-syphilis and other organic conditions and also in hysteria and compulsion neuroses."

"In the various outlines of the crises different observers have given but few details of bodily position, and even fewer have paid attention to the mental content of the compulsive acts or of compulsive thinking."

"These oculogyric crises are not to be regarded as isolated phenomena, but the organism as a whole should be viewed at all levels of its adaptive capacity, vegetative, sensorimotor and symbolic."

After referring to pertinent works of various authors dealing with lesions of cerebral tracts, the writer points out that "where there are so many localizations it is evident that there is poor comprehension of the cerebral mechanics." He directs his study therefore to the "purposeful pattern, if any, of the symptomatology." The study of the impulses and activities of primitive man shows the important place of ritual, "the essence of which is compulsion." "Thus an oculogyric crisis, *i. e.,* a compulsive activity, contains a bit of ritual."

"A compulsion is a substitution phenomenon. Freud's conception here is far advanced over any other for comprehension."

Then follows an interesting discussion of the symptoms, eye movements, classification, periodicity and severity of the eye spasms, with clear photographic illustrations from patients, and a comprehensive reference to other observers.

The remaining three chapters dealing with symptomatology and the last one dealing with psychopathology are intimately concerned with the formulations of Freud. Neither the schematic diagram of mental apparatus (modified from Freud) nor the author's skilful formulations involving the metapsychology of the former has tended to clarify in the mind of the reviewer the significance of these phenomena. The volume closes with a very full bibliography of the literature.

E. P. LEWIS,
Toronto Psychiatric Hospital.

The Human Aspects of Unemployment and Relief. By JAMES MICHEL WILLIAMS, PH. D. (*Geneva, N. Y.: University of North Carolina Press, 1933.*)

While this volume brings comparatively little that is new to the psychiatrist, dealing actively with community mental hygiene problems, it presents a complete, careful, and thorough analysis of present-day community resources which deal with unemployment and relief. It is primarily a book of guidance for the social worker engaged in family case work, but contains much that is food for thought, in bringing out the points where present day methods of social work often fail. While frankly critical of many organizations, which through failure to fulfil their obligations, play havoc with their clients, the author goes on to offer constructive practical solutions, which if accepted would do much to enhance the value of many social agencies that are at present failing to secure results through the disregard of the human side of their responsibility.

It is a book that offers a broad appeal to all engaged in any phase of social work.

ERIC KENT CLARKE,
University of Rochester.

Manuale di Antropologica E Psicologia Criminale. Applicata alla Pedagogia Emendativa, alla Polizia ea al Dritto Penale E Penitenziario. (*Manual of Criminal Psychology and Anthropology.*) By B. DI TULLIO. (*Rome: Anonima Romana Editoriale, 1931.*)

This 350 page manual of criminal psychology and anthropology presents well-systematized material full of social interest and human appeal.

The body of the work is preceded by an introduction which not only defines the object of this study, based upon observation and experiments on more than 15,000 criminals over a period of ten years, but clearly delineates the function of criminal anthropology in science as well as in social life. This function consists "in the study of all those individuals who, because of their abnormal personality or constitution present a more or less serious predisposition to anti-social behavior so that it may seek the causes of such tendencies, fight them, and impede their development from their initial manifestations."

Criminal anthropology in its developmental phases; the Lombrosian, the pre- and post-Lombrosian periods (the latter carrying us up to the present) is briefly sketched. No important contribution is ignored, every point of view is considered, and every controversial question, is dealt with.

The author now introduces the main subject by giving the reader a cursory historical glance of the concept of "Criminal Constitution." This concept, says the author, though traceable to Hippocrates, was better interpreted by Malpighi, Morgagni, Baglivi, Bichat, Cabanis, Pinel, etc., and ultimately clarified and perfected by DeGiovanni who was the first to perceive its full significance and implication.

Theories on the etiology of crime are now mentioned and particularly stressed are those of Benedict, who believes that crime is caused by a form of weakness of the nervous system; Marro, who believes it to be due to defective nutrition of the nervous system; Bonfiglio, who also supports the theory of weakened inhibitory centers; Maudsley, Morselli, *et al.*, who believe in a criminal neurosis; and finally, Lombroso, whom the author justly regards as the one, who, by clarifying and fixing the biological causes of crime, placed it on a scientific basis.

The essential and secondary factors of crime are now presented and discussed, with particular emphasis upon drug addiction, alcoholic excesses, constitutional toxemias, such as tuberculosis and syphilis, physical and psychic traumas, and finally, the influence and bearing of endocrinology and environment on crime.

The next chapter is devoted to the examination of the criminal's physical and psychic characteristics, the methodology used in bringing out these morphological and functional qualities and their value as symptoms pointing to the development of criminality.

The following five chapters are given to the application and results obtained by the application of this method of investigation to the study of criminals according to the classification subsequently submitted by the author.

An important discussion is that on the biological determinism of the principal and commoner offenses. The author insists that the dangerousness of a criminal is a bio-social phenomenon which is always the resultant of a fusion of the predisposition to crime with the secondary elemental factors, particularly the environmental. He maintains that if we observe the intimate life of the "criminaloid" type, we will find more habits which are true equivalents of crime. These offenses, though not punishable under the established penal codes, are nevertheless just as harmful to society. He holds that the reason why notorious criminals usually come from the poorer class is due to the fact that the rich, because of environmental differences, are often able to compensate for these constitutional anomalies by hygienic measures imposed upon them since infancy so that their criminal tendencies express themselves in non-punishable crimes such as usury, appropriation of funds, systematic adultery, frequent duels and similar offenses.

The last two chapters are of special value to all scientific students of crime. The first compares the various classifications of criminals now in

vogue, particularly that of Vervaeck and that of Ottolenghi upon which the author's own scheme is based. He divides criminals into two classes:

1. *The Occasional.*—Here the crime is accidental or unpredictable. These are pseudo-criminals in whom poverty, ignorance, misery, etc., may act as causes. They should show no characteristics of the true criminal. Political offenders are of this class.

2. *The Constitutional.*—These are predisposed to crime because of inherited somatic and constitutional characteristics with resulting psycho-physical abnormality. Under this category we get the criminaloid type in whom the etiology though more complex and elusive is bio-social none the less—the psychopathies, neuro-psychopathies, atavistic, mixed, and insane types.

In this chapter the innumerable variety of criminals are masterfully depicted and classified. It leads admirably to the final chapter which discusses the therapeutic and prophylactic measures in dealing with crime.

Briefly we may summarize the author's contribution. If we ignore the almost negligible number of accidental criminals, that is, those in whom crime is not the result of a desire to do harm, is not premeditated and therefore not criminally intended, we find all of the true criminals to be possessed of what the author terms "criminal constitution." The "criminal constitution" manifests itself as an active or latent predisposition to crime, a predisposition which develops under the influence of secondary factors, whose action is to accentuate it and thereby favor its exteriorization.

The author maintains that crime prevention is based upon the attenuation of this predisposition, as well as its deviation into socially constructive channels. This can be accomplished through hygienic means instituted at an early age (as soon as this predisposition is noticed), more humane and scientific treatment of those under the jurisdiction of constituted authorities, and a more active collaboration between corrective and preventative institutions. Encouraging results follow wherever this cooperation and understanding are achieved.

The author rightly holds that what is needed for the solution of the problem of crime and its prevention is a greater number of properly organized social agencies and institutions for the guidance and correction of youth, with active cooperation between them and the legislative, judicial and law enforcing bodies.

E. A. MANGINELLI.

The Nature of Human Conflicts. By A. R. LURIA. (*New York: Liveright, Inc., 1932.*)

The author of this "objective study of disorganization and control of human behavior" is professor of psychology at the Academy of Communistic Education and research associate at the State Institute of Experimental Psychology in Moscow. The translation of this book from the Russian provides a welcome introduction to the psychological research being carried on in the U. S. S. R. at the present time. The translator and editor of the present edition (W. H. Gantt) worked as collaborator in Pavlov's laboratory

in Leningrad from 1924-29 and thus brings to this task a first-hand knowledge of contemporary Russian experimental psychology.

The sub-title of the book, "Emotion, Conflict and Will," enumerates the concepts with which the three main sections of the work are directly concerned. Although apparently somewhat diverse, these three categories are knit closely together by a progressive sequence of experiments and the book emerges as a well-integrated experimental treatment of the major problems of the disorganization and control of human behavior.

The method employed is described as "the method of associated motor reactions" and involves the simultaneous recording of speech reactions to selected word stimuli, associated pressures of the active right hand, movements in the passive left hand, and changes in respiration.

Perhaps the reader's first impression is one of surprise that such complex problems of the human personality should be attacked so optimistically with such simple laboratory methods. It is not usual to expect the intricacies of "conflict" and "will" to reveal themselves openly in the scratches of a kymograph needle. (There are reproductions of more than a hundred kymograph records throughout the book.) In fact, a superficial examination may give the impression that we have in this book merely another example of the "muscle-twitch" variety of experimental psychology.

However, as the sequence of experiments is followed through its development and elaboration and as one recognizes that the author is using his graphic findings merely as a symptom-reflector which proves of assistance in the re-interpretation of truly psychological phenomena, there comes the refreshing impression that it is really possible to make strictly experimental observations in the laboratory setting which will give verifiable information concerning the complex problems of the dynamic personality in action.

The preliminary series of experiments was carried out on groups of subjects known to be suffering under acute emotional strain. Students awaiting examination by the "purgation" committee (which was engaged in "cleansing" the higher schools of those inimical to the revolution), students facing critical academic examinations, criminals under arrest for murder, and such groups, were made available for examination. Laboratory observations on these subjects, when compared with similar observations on normal control subjects not only provide the author with a definite symptomatic picture of acute emotion as reflected in certain voluntary motor systems but leads to the conclusion that one of the important factors causing the disorganized behavior characteristic of acute emotion is the mechanism of "conflict."

A further series of experiments is then carried on in which "artificial conflicts" are produced either by introducing a conflicting tendency after establishing a strong "set" on the part of the subject, or by inserting conflicting suggestions under hypnosis. The usual clinical criteria are used to verify the presence and stability of the "experimental conflict." In some cases the conflict was so acute and persistent that the author feels justified in describing the condition as an "artificial neurosis." The compulsive behavior and the disorganized neurotic phenomena obtained in this way are subjected to detailed analysis in regard to their associated motor symptoms.

An interesting demonstration is made at this point of what the author calls "experimental psychoanalysis." This consists of a graphic presentation of the motor phenomena accompanying the shift of an acute emotional complex from unconscious to conscious during free chain associations. The fact that the exact nature of the complex is known to the experimenter but not to the subject (post-hypnotic amnesia) allows for "experimental" analysis.

The associated motor phenomena observed during "experimental conflicts" show that there is a considerable difference in the extent to which the conflict disrupts organized behavior and it is further seen that this difference depends on the point at which the conflict occurs. For example, in one experiment on cases suffering from motor aphasia, it is shown that when the conflict is shifted experimentally so that it occurs early during the receptory or central phase rather than later near the motor terminal, the resultant speech behavior is smooth and well-organized. From this and similar evidence the author is led to assume the existence of a "functional barrier" between the central "coupling-up" and motor "expressive" apparatus which makes possible that central delay and preparation essential for the organization and control of behavior.

The author refuses to "neurologize" this "functional barrier" and offers some caustic comments on the comedy of errors played by certain "objective" psychologists in their vain efforts to secure a firm foundation in the shifting sands of neurological theory. Like Pavlov, he scoffs at the naive adoption of conditioned reflex mechanisms for the interpretation of complicated forms of human behavior. He prefers a frankly psychological interpretation of the psychological evidence at hand.

There follows in the final section of the book a series of experiments designed to show how the insufficiency or absence of this "functional barrier" in certain pathological groups (subnormals, hysterics, neurotics, etc.) and in very young children makes control of behavior difficult or impossible. Experiments with children of different ages provide indications of the manner in which the "functional barrier" develops genetically. It is shown that at any level a direct "effort of will" is quite futile in controlling behavior. The child succeeds in the organization of his own behavior only in so far as he is able to establish and make use of external supplementary stimuli. Later, "internal" stimuli are substituted and in this connection emphasis is place on the important rôle of language in the development of this "self-control."

Any general evalution of the book must recognize the practical contribution which it makes to many fields of marked psychological and psychiatric interest. To mention only a few, there are the proposed experimental methods for: the "objective diagnosis" of participation in crime; the early diagnosis of neuropathic tendencies; the measurement of "neurodynamical" age in children; and the shifting of a "conflict" away from the motor apparatus with consequent disappearance of disorganization from the behavior. The author's marked psychiatric interest and the inclusion of so much clinical material in his experiments do much to enhance the practical importance of his contribution.

On the theoretical side, we are told that the author likes to style himself a "Marxist—Materialist—Psychologist." However, this does not mean that he can be "labelled" as belonging to any of the more vociferous "schools" of contemporary psychology. Neither behaviorist nor psychoanalyst, Prof. Luria gives evidence in this book of being a strictly experimental psychologist with a marked clinical interest and a genius for stimulating interpretation. Prof. Adolf Meyer, in his foreword to the book, characterizes it as "a genuine contribution to psychiatric work and interests." As an example of careful research of the laboratory type, it can further be regarded without hesitation as an important contribution to experimental psychology.

C. ROGER MYERS,
Dept. of Psychology, University of Toronto.

The Neural Energy Constant. By JOHN BOSTOCK. (*London: George Allen & Unwin, Ltd., 1931.*)

Dr. John Bostock's "Neural Energy Constant" is a decidedly new and interesting approach to the study of the basis of consciousness. In an attempt to explain the evolution of consciousness, Dr. Bostock postulates the existence of three distinct factors: (1) Simple awareness (common to the lowest protoplasmic types); (2) Coarse adjustment (essential in keeping the organism to a definite purpose); (3) Fine adjustment (associated with the more complicated activities). The development of consciousness is thus described as one of gradual evolution. The primitive state is pure awareness quickened by the urge to adjust. This urge to adjust primarily to the primitive sensations of pain and extremes of heat and cold resulted in the evolving of a center for emotion which served as a coarse adjustment to direct the organism towards the goal in view. This came about as the result of the formation of slowly discharging cells which were associated with feelings of comfort and discomfort and were utilized for such needs as hunger and reproduction. A further extension of the neural system then became necessary as a result of more complicated modes of life. This center is called the organ of fine adjustment or discrimination and completes the evolution of consciousness to date. Only when these three centers are in correct functional apposition does complete consciousness occur. The awareness and emotion centers are localized in the region of the optic thalamus, and that for fine adjustment in the *cortex cerebri.*

The neurone is described as extremely fatigable under which condition it dissociates, becoming inert until it has had time to recuperate. Thus we have the necessity for sufficient insulation of the neurone, a minimum number of neurones in use in any moment of consciousness, and a constant amount of neural energy engaged in the circuit which subserves consciousness—the neural energy constant.

Sleep is described as a dissociation of the centers of awareness, emotion and fine adjustment; while the dream is a state of partial consciousness in which the three centers are in incomplete apposition so that one or the other may predominate.

The point of view offered with regard to instinct is that only the components are innate. The actual basis consists of the urge to adjust, and its particular form is a matter of individual emotional setting and fine adjustment associations rather than of instinctive tendency.

The germ of the hypothesis grew out of Dr. Bostock's attempts to understand more fully the picture of dementia præcox. He suggests that certain types of mental disorder show the influence of changes in the neural energy constant and describes the changes in consciousness that are presented in mental disorders as dissociations of the three centers.

It is true that experimental evidence is lacking for substantiation of some of the points of view offered, such as the superior intellectual ability discussed under hypnosis, the localization of the various centers as well as the dissociations underlying morphine and alcoholic addiction or katatonic and epileptic episodes. The greatest criticism that can be offered is that the existing facts, as we know them, have been fitted into the theory, a method which is always open to question.

The book on the whole, however, is interestingly, carefully, and fearlessly written and certainly is founded on a more plausible basis than is the theory of psychoanalysis which has so claimed our attention. While the point of view may appear radical, it has much promise and is worthy of our attention.

G. WILSON SHAFFER.

Neurobiologie de L'Hallucination. By RAOUL MOURGUE. (*Brussels: Maurice Lamertin, 1932.*)

In his foreword to this work Henri Bergson points out some of its salient features. It represents the study of some fifteen or twenty years. Its scope is represented in the bibliography at the end of the volume, comprising 1081 references. These are the publications which the author has consulted. Some six thousand others of which he took account are omitted as not having sufficient bearing upon his subject. He brings under contribution the whole domain of psychology, biology and medicine. The author declares his indebtedness to M. Bergson, whose biological theories (*Matière et Mémoire*, 1896) offered the point of departure for his own studies.

By "hallucination" as discussed in this work, the author means "true hallucination," namely, "that which in its objective character differs in no way from normal perception."

Chapter I deals with the history and evolution of theories of the pathogenesis of true hallucinations. Prior to the 19th century the subject of hallucination had hardly been scientifically broached.

In Chapter II the author discredits the classical theory that hallucinations represent essentially the intensification of "mental images."

Chapter III discusses "Hallucination and depersonalization." The word "depersonalization" is used only in the etymological sense, implying "*un trouble profond de la personnalité,*" with no attempt to define the precise nature of this disturbance. Analogous descriptive terms of other observers are quoted. Kandinsky speaks of *perturbation* or *obscurcissement;* G. Ballet of

désagrégation; Parish and Lipps of *dissociation;* Morton Prince of *abstraction* or *dissociation;* Berze of *rétrécissement;* Wernicke of *disorientation* and *sejunction.*

"Depersonalization appears to be a necessary condition but not of itself sufficient to produce true hallucination."

Then follows (chapter IV) a long discussion of motor attitudes in the genesis of hallucination. Referring again to the "classical" theory as expressed by Wundt—"Hallucinations are memory images which are distinguished from normal images only by their intensity"—Mourgue points out that the traditional habit has been to interpret hallucination solely in terms of *perception;* and to think of *perception* as a sort of isolated cognitive affair resulting from peripheral stimulation, without sufficient appreciation of the reflex or motor components. This conventional habit of reasoning appears to be an unfortunate survival of the *intellectualistic* attitude which prevailed so long in psychology, a hereditary handicap chargeable to its philosophical parentage.

Ribot (1879) foreshadowed contemporary views. "Le mouvement est un élément de la vie psychique tout aussi bien que la sensation on l'idee." And Féré (1887): "Toute excitation détermine un mouvement auquel paraissent prendre part tous les éléments contractiles de l'organisme, et *ce mouvement semble constituer essentiellement le charactère objectif de la sensation.*"

Drawing upon psychologists, physiologists, neurologists and psychiatrists from England, France, Switzerland, Germany, Belgium, the author sums up: "In sensory phenomena, and in a more general way in the psychic life itself, there is no process which has not its motor correlate. "Féré speaks of the "physical signs of hallucinations," and various authors have remarked that motor attitudes and expressive movements may be better evidence of hallucinatory experience than the verbal responses of the patients themselves. These "motor attitudes" constitute the necessary means of adaptation to the external world and are regarded as integral components of the sensory experience.

Spatial objectivity which characterizes true hallucinations according to the author's definition, depends on the one hand upon the hypothesis of depersonalization which accounts for objectivity with reference to the *self,* and on the other, upon motor attitudes which account for objectivity with reference to the *body*—projection *par excellence.*

The author proceeds to a study of the physical basis of hallucination, and finds its primary conditioning in particular states of the organo-vegetative system—an observation, he recalls, which Baillarger had made as early as 1842. Of capital importance in this connection are the observations reported by Head in the Goulstonian Lectures (1901). Head's material—cardiac and pulmonary cases without psychoses—was particularly favorable for the study of the pathogenesis of hallucinations. The sole factor always present in the hallucinatory cases, Head found, was *reflected visceral pain* of considerable intensity and duration. Mourgue reviews at length the classic investigations of Head, likewise the work of the brothers Jaensch on *eidetic images,* and concludes that "thanks to the light which the researches of the brothers

Jaensch have thrown upon the interpretation of Head's cases, one may say that the phenomenon of true hallucination has its primary origin in a dysregulation of the organo-vegetative system (*i. e.*, of the instinctive sphere)." Here indeed must be sought the physical basis of mind.

A point which can never be too much emphasized is that an hallucination cannot be studied as an isolated phenomenon, but only as an aspect of a global psychic state. This fundamental thesis the author well documents in the profound study presented in this book.

C. B. F.

Ueber Ideenflucht. By LUDWIG BINSWANGER. (*Zürich: Art. Institut Orell Füssli, 1933.*)

This 200 page study appeared first as a series of articles in the Swiss Archives of Neurology and Psychiatry, now brought together in monograph form.

The author has chosen a common and generally recognized symptom in clinical psychiatry, and explored it not alone as the expression of a given psychopathological process but in a much broader sense as an index to a particular kind or mode of human experience.

Using the clinical psychiatric concept as a starting point the investigation is pushed beyond the confines of so restricted a horizon and becomes a psychological study of a personality type, eventually an anthropological enquiry.

"'Flight of ideas' is no longer therefore merely a question of psychological definition or clinical diagnosis. . . . . To be sure there is advantage in dealing with the symptom in this way, in that it can be added to or subtracted from other symptoms, calculated in the clinical reckoning and assigned its place in the 'manic triad' syndrome. . . . . Such a view however does not furnish an understanding of what the flight of ideas signifies in human living and being as a totality (im Ganzen des Menschseins). . . . . This understanding can be gained only by means of the existential-anthropological viewpoint."

Individuals with the characteristic of idea-flight simply represent special human types, distinguished from the so-called average man and from the philosophical or reflective varieties.

In the psychopathology of idea-flight the author names the following constituents:

1. Optimistic mental set.
2. Sense of clarified limitless thought sphere with facilitated movement of the thought stream.
3. Accelerated inner tempo.
4. Thought objects not sharply defined.
5. Confluence of meanings, as seen in the idea-flight of dreams.
6. Dominant impulsion to social contact, to communicate, to turn away from self.

The study is well documented and is provided with a full subject and name index.

C. B. F.

Human Sterilization. By J. H. LANDMAN, PH. D., J. D., J. S. D. (*New York: The Macmillan Co., 1932.*)

In this book is made available for the first time a great body of timely and reliable information upon the subject of sterilization in all its social, medical, biological and legal aspects. It is not a book of propaganda. The author's attitude is thoroughly conservative. The objective of any sterilization program, as he sees it, should be "the prevention of the multiplication of those potential parents carrying degenerate hereditary qualities, provided such socially inadequate persons have inherited their defects and may transmit them to their children."

After giving some account of the eugenics movement, noticing the various methods that have been proposed for preventing the increase of the socially unfit, and presenting data as to the prevalence of mental disease and defect in the United States and the economic waste entailed, the author proceeds to a historical review of the movement to legalize sterilization.

This movement is just a quarter of a century old. To Indiana goes the credit of enacting the first sterilization law (1907). Thirty states in all have passed such laws, and in 27 states these laws are valid today. The states showing the highest numbers of legal sterilization to January 1, 1932, are:

California	7548
Virginia	658
Kansas	657
Michigan	629
Oregon	576
Minnesota	508
Nebraska	386
Wisconsin	248
Connecticut	158
Delaware	141
Indiana	120

A total of 12,145 persons (male, 5613; female, 6532) have been sterilized under the several state laws.

The legal aspects of the problem are dealt with in sufficient detail. Here are recorded some of the epochal legal decisions on sterilization, beginning with the celebrated Buck *vs.* Bell case in Virginia, involving a feeble-minded girl who had given birth to an illegitimate feeble-minded child, and who was herself the daughter of a feeble-minded woman. The constitutionality of the law under which this patient had been sterilized was upheld by the Supreme Court of the United States (1927). In the opinion of Mr. Justice Holmes occurs this historic paragraph.

"There can be no doubt that so far as procedure is concerned the rights of the patient are most carefully considered, and as every step in this case was taken in scrupulous compliance with the statute, ... there is no doubt in that respect the plaintiff in error has had due process of law.

"We have seen more than once that the public welfare may call upon the best citizens for their lives. It would be strange if it could not call upon

those who already sap the strength of the state for these lesser sacrifices, often not felt to be such by those concerned, in order to prevent our being swamped with incompetence. It is better for all the world, if instead of waiting to execute degenerate offspring for crime, or to let them starve for their imbecility, society can prevent those who are manifestly unfit from continuing their kind. . . . . Three generations of imbeciles are enough."

A section of the book is devoted to a discussion of the various types of mental abnormality, and the author attempts to indicate the salient features of what is known about inheritance in these conditions, and of what may and may not be expected from such eugenic measures as sterilization. The various surgical procedures are described, together with the reported results, physiological and social, which have been for the most part favorable. As a rule little or no change has been noted in their sex life by persons operated upon. When a definite difference has been observed it has more often been favorable than otherwise. The California experience is quoted to correct two popular phobias with regard to sterilization, namely, that increased promiscuity and spread of venereal disease would be natural consequences. California has not found such consequences.

That the proceedings of legislatures, zealous for the improvement of the race, do not always exemplify the higher psychic levels which sterilization is calculated to promote the author sadly records. In 1929 a bill was introduced in the Missouri legislature providing for the sterilization of persons "convicted of murder (not in the heat of passion), rape, highway robbery, chicken stealing, bombing, or theft of automobiles." The bill was defeated.

The book closes with several appendices containing statistical data and tabulations of state laws, an extended bibliography including numerous legal citations, and an index.

C. B. F.

In Memoriam.

MAX ERNST WITTE.

Dr. Witte was born in a suburb of Berlin, Germany, on the 31st of January, 1859. He died following a vascular insult to the brain, January 29, 1933, at his home in the Clarinda State Hospital for the Insane, of which he had been the superintendent for 34 years. He came to America when six years of age and attended school in Bellevue, Iowa. A little later he studied theology for two years and business methods for one year, after which he entered and completed the regular course in the Medical College of the State University of Iowa, receiving there the degree M. D. in 1881.

Almost at once after graduation Dr. Witte became assistant physician of the Iowa State Hospital for the Insane at Mt. Pleasant and continued this work until 1898, when he was appointed superintendent of the state hospital at Clarinda, Iowa. He was twice married and had two children.

Dr. Witte was president in 1919 of the Iowa State Medical Society, and was a member of the American Medical Association and of The American Psychiatric Association.

Of a quiet, friendly nature, yet with courage and perseverance, and with a friendly interest in varied human affairs, he exerted a constant influence in the state of Iowa toward sane methods in the treatment of the insane. He encouraged the establishment in 1919 of the State Psychopathic Hospital as the basis of the Department of Psychiatry in the University of Iowa. As an administrator, he was cautious, far seeing, and a model of integrity.

Dr. Witte's contributions to medical literature were unassuming, but always sagacious and lighted with quiet humor. A characteristic example was his " Recollections of the Care of the Insane Thirty-four Years Ago," read before the Conference of the State Board of Control in 1915, in which he presented an interesting and start-

ling picture of the condition of the patients in a hospital for the insane during the late decades of the 19th century.

That the insane of his state are living today under better conditions and with measurably improved chances of recovery is due in part to his quiet influence and persevering effort. He did not grasp after fame; but he used his powers to the limit in serving his generation, which is the true measure of greatness.

ANDREW H. WOODS.

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